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THE CRITERION OF PSYCHOSIS *

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AT a time when a thorough and much needed revision of our nosologic schema is in the air—although not *on* the air as yet—it seems accordant to call attention anew to a feature of psychosis that is fundamental in insanity and in the diagnosis thereof, but which, none the less, has been unduly neglected by theoretical psychiatry. Practically, by the legal profession, by jurists, and by rating-boards, it has long been given at least a modicum of its real psychiatric value. This indispensable factor or aspect of every true psychosis—possibly with one exception presently to be noted and explained—is *mental deterioration, dementia,* and especially deterioration of "the intellect". We are leaving out of our present remarks almost wholly affective or emotional deterioration, not because it is any less important than intellectual regression (for frequently it is of more import than the other), but simply because of our present lack of scales for determining and for measuring these quantitative changes in the affect. In very many cases, perhaps indeed always, intellectual deterioration may be used as a reasonably accurate index of affective deterioration also, especially in the way of its flattening. And then, too, this latter sort is of narrow and infrequent occurrence (hebephrenic precox, paresis, arteriosclerotic psychosis) compared to that of the intellectual type of deterioration. It is of less importance, too, because it is less concerned in the long run

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in practical incompetency and in one's personal welfare in a social community.

The particular need of this suggested additional stress on deterioration, of course, is in the diagnosis of borderline or doubtful psychiatric cases. Many of these prove on adequate examination to be merely feebleminded. There are, however, many more, a group much larger than the moronic group, as to whom, at least for the time, much and often insoluble doubt of the diagnosis persists. As the newer ideas of continuity between the neuroses, the psychoneuroses, and the psychoses become generally accepted, it frequently is a purely arbitrary choice whether to call a given case a psychosis or a psychoneurosis. In the hospital where I work we could readily make a list of very many out-patient cases who eight years ago or less were adequately diagnosed as psychoneurotics who today would be quite as adequately called psychotics. Certainly it would largely be the more or less obvious intellectual deterioration that would impinge as an apperception, so to say, on the diagnostician's plastic neopallic neurones, so to speak, and validate the change of diagnosis.

Did one more commonly use intellectual deterioration as the deciding criterion, the diagnosis would be more certain; more scientific (because determined by an "instrument of relative precision"); and more useful in the patient's affairs and life in general. Of this instrument of relative precision and its sanctions and its mode of application I had the honor of speaking noisily at the eighty-second annual meeting of this Association in June, 1926, and it was published in our *Journal* (*The American Journal of Psychiatry*), Vol. VI, No. 4, in April, 1927. The unscientific flight of what we call time prevents rehearsal of this matter now, so I pray ye take heart. I only insist here that the determination and the measurement in question is a progressive step, however short, toward the goal of compelling psychiatry (even against its will it seems at times) to line up with the other sciences as a more or less measurable and therefore exact and reliable science. This bold, but after all not unreasonable, hope I set forth at quite scientific length in the *Journal of Mental Science* of April, 1928.

On both the validity and the relative ease of this deterioration-determination by a psychiatric psychologist, alias a psychological psychiatrist, I cheerfully stake my word.

At least as good as any brief formulation of the mental process is that of H. Höffding: "The *unity* of mental life has its expression not only in *memory* and in *synthesis,* but also in a *dominant*

fundamental feeling characterized by the *contrast between pleasure and pain,* and in an *impulse,* springing from this fundamental feeling, to *movement and activity.*" Here are a dozen complex concepts that are probably the most essential in descriptive psychology. Let us see, very inadequately indeed, how mental deterioration affects some of these respective aspects of mind, the ultimate mystery we know to be our own selves.

The "unity", individuality, is most seriously affected by deterioration. In precox, for example, the patient spontaneously speaks of himself as a different and mostly new person—as strange to himself as to others. Of course, after a time the "new" personality on a lower plane becomes familiar and unified again.

"Memory" is primarily deteriorated in psychosis, constituting one of the two most basal deficiencies in mental deterioration. Without memories the individual is only a machine and consciousness has only surface, with no depth to its "stream".

"Synthesis", what Kant called the "transcendental unity of the understanding", is deeply injured in deterioration so that thought becomes sometimes a hodge-podge without constructive powers. Paresis exemplifies this, but no better than precox or a bad senile psychosis.

How "dominant" the affect normally is, no psychiatrist needs to be reminded, and how flattened out and servile it becomes in mental deterioration we all appreciate. In normal conditions it supplies nearly all our motivation (despite the objections of the idealistic optimists). The affect of course is absolutely "fundamental" for it provides the drive, the "libido", of life itself in its last analysis, and is primary, phylenically speaking, to intelligence.

The lessening of the "contrast between pleasure and pain", seen so often in deterioration, leads to such unpleasant behavior as starvation, picking one's finger-nails off the fingers, swallowing pounds of nails, bedsprings, stones, etc., as well as to cruel errors of judgment of a less dramatic kind, such as various ultra-schizoid behavior. On the other hand, pleasures are sought no longer save as habitual or automatic instinctive actions. An imaginary suggestion from God is enough for self-torture or for matricide.

The "impulses" actuated by the pleasure-pain balance or imbalance lessen with deterioration, or they may be increased and misdirected, giving us the behavioristic range between total seclusiveness and catatonia and the wild frenzy of acute mania or of agitated melancholia. And in a corresponding way the mental

"activity" may be altered all the way from a relatively thoughtless stupor to the hurrying, scarcely understandable productivity of a manic logorrhea. The impulsive acts of the psychotic epileptic at times startle the world.

Another way of viewing the psychoses, with the intent of bringing out the wide occurrence, and also the qualitative meaning of deterioration in general terms, is to set down the chief symptoms of mental derangement and to observe them with this search in mind:

Amnesia	Facetiousness	Negativism
Apathy	Fugues	Retardation
Aprosexia	Hallucinations	Schizophasia
Autisticism	Hyperprosexia	Seclusiveness
Catatonia	Ideational flights	Silliness
Clouding	Impulsiveness	Starvation
Delusions, fixed	Incoherence	Stereotypy
Depression	Incompetency	Stupor
Disorientation	Insight-lack	Suggestibility
Distractibility	Irascibility	Suspicion
Episodes	Irrelevancy	Unreliability
Euphoria	Mnemasthenia	Verbigeration
Evasion	Mutism	

Of these two score and more of the symptoms of psychosis (several in the accompanying list include a group of items), about 70 per cent may properly be said to be peculiar to insanity, the remainder having part in the minds and behavior of unstable individuals properly within the range of normality. Of those proper to psychosis, at least half are the products of intellectual and affective deterioration, most of them of the former.

One sees foom such an analysis, however cursory and psychologically inadequate, that, speaking in general terms, intellectual deterioration involves practically every group of intelligent process and behavior. It is not only a matter of a loss of the power of mind-direction and of experience-recall, but it disturbs and undermines essentially the entire personality and its normal life. It is because of this that its recognition is quite indispensable in every adequate consideration of a case of true mental derangement. Right here, it seems to me, is the only criterion of psychosis at once definitive, actual, and available by determination and measurement. In other words, such determination is both necessary and expedient; and it is readily available.

So long as a group of mental derangements, mostly temporary and, properly speaking, abnormally "functional", are included in

our lists of psychoses, we will have to think of deterioration as a temporary condition sometimes, as well as one that is permanent. Most common of these, perhaps, are traumatic delirium, cerebral embolism, meningitis, Sydenham's chorea, alcoholic deliria and hallucinosis, the confusion and delirium from opium, metals, pellegra, and from somatic diseases. I think it obvious that it would add to scientific clarity and precision if such conditions were dropped from the psychotic nosologic groups, for in a general sense they are not insanities, but mental symptoms mostly delirious and confusional, that properly are but part of the respective somatic conditions showing themselves by more or less extrinsic disturbances of the brain, and for a time relatively brief. "Temporary insanities" have in practice proved to be a reproach to psychiatry, and confusion, delirium, hallucinosis, etc., might also logically be omitted owing to their tendency, as well as that of "temporary insanity", to confuse the concept of psychosis. In a general sense the insanities, psychoses, are diseases of the mind while the others are more or less intrinsic accompaniments, or factors even, of numerous diseases primarily more largely somatic.

For the sake of relative logical completeness, five or six of the dozen or more factors of mentation most often observed to be deteriorated, may be briefly noted. We may reasonably assume, I think, that the greater part of human intellectual regression is included in these following changes:

First and perhaps the most characteristic change is a lowering of the power and the habit of voluntary attention, leaving it "spotty" and unreliable. The deteriorate or dement frequently speaks of this spontaneously, noting that his "mind" of late goes blank or else wanders from the associational line when he tries to think or otherwise to attend to his work or play. In physiological terms perhaps part of his control has stepped "down" from the cytopenic inhibitory neopallium to the archepallium, his attention becoming therefore more "reflex" than before.

Second, there is a virtual or a real lessening of the power of memory and of recall, another practically universal factor of intellectual deterioration very apt to be appreciated easily by the sufferer and readily reported.

Third, there is a derangement of conceptual association in general, giving rise to a tendency to avoid concrete thought, leading, as Bleuler points out, to increased generalization often erroneous, and often going so far as to abolish the power of inference, to

"think the therefore"—the basal mental criterion of rationality. Irrelevance, incoherence, dementia, are the results.

Fourth, under the whip of suspicion or of anxiety or of the wish to escape control, a process, often very ingenious and persistent at first (*e.g.*, in paranoid precox) is seen, of the nature of evasion. The absence of this factor from traumatic brain-injury cases, from epilepsy, from cerebral tumor cases, often helps us in differential diagnosis.

Fifth, deterioration of a sort often shows the derangement of perception that results in true hallucinations, that is false perceptions about whose falsity the patient has no insight. In most if not all illusions, on the other hand, insight is retained in this respect, and this is the case in the pseudo-hallucinations so common in the temporary deliria where there is no true deterioration in the sense in which we are employing the term.

Other psychological deteriorations, and not including several highly important affective items, are: the lessening of personal purposiveness, the very essence of personality; abstraction from the concrete interests of life; an increased psychobiologic mechanization; an exuberance of imagination more or less uncontrolled; a slowing of the mentation (not including retardation proper) and encephalitic inertia of thought; impairment of judgment and of inference; sensory changes (malaise, headache, dizziness); atavistic perversions; and terminal dementia to almost the vegetative status.

But of all these, the first two, aprosexia (lessening of attention-control) and mnemasthenia (lessening of retention and of recall) are most important practically, the most conspicuous usually, and the commonest, perhaps.

To describe the specific deterioration-picture of the various psychotic types clearly would be to fairly well describe each type, a third-year medical student task. But some of the forms of real insanity have deterioration-syndromes, complexes that may be characterized in a few words,—as one might tell a boy the more striking differences between typical pictures by Turner, Landseer, Franz Hals, Whistler, Velasquez and Rembrandt; or Mozart from Beethoven and Wagner from Herbert.

The old-fashioned general paralysis, paresis, used to show at times, one might say, more universal or general, all-around deterioration, perhaps, than the other psychoses, the term dementia getting herein its typical example, even to a creature, short-lived, fortunately, whom one hates to recall to mind, and now not so often seen.

The senile psychosis is not far behind, but with less of the forbidding regressions of somatic instincts. It would not be too easy to find a form of mental deterioration not observable at times in the senile group.

Schizophrenia shows in general a preponderance of the affective deteriorations, but with plenty of intellectual defects too. Disturbances of perception and of the association of ideas are prominent among these latter.

Epileptic psychosis, on the other hand, shows more of the deterioration of the knowing aspects of mind, the basal processes of thought and of memory being most impaired. But, of course, affective outbreaks are all too common with the direst of social consequences.

Encephalitis lethargica knows too often a psychosis whose impairment is hard to characterize because of its protean variety. But it is very deep-lying, thorough, and pervasive.

In brain-tumor cases the intellectual deterioration (aprosexia, etc.) not seldom is the *first symptom* of the impending cerebral calamity, and usually is well realized by the patient and wondered at. This deterioration usually is almost wholly of the intellectual part save as this actuates emotional states also.

The psychosis of amentia is typically episodic, the episodes showing deterioration well mixed of intellectual and of affective factors, and permanent.

Manic-depressive psychosis for our discussion is distinctly a special case and requires explanation. Its impairment so far as qualitative is largely in the affective aspect of the mental process. As everybody knows, it especially involves the basic "contrast between pleasure and pain" of Höffding, ranging between a deep melancholy and the most excited mania even to frenzy. But the major deterioration is in the quantitative direction, making this psychosis almost unique in this general regard. There certainly is, however, a slowly progressing intellectual impairment in this psychosis, and I have never seen a case of several episodes in which it could not be demonstrated by the proper method of analysis. Each time the intellect sags a little.

In the common "mixture" usefully termed by Bleuler and Brill the schizoid-manic psychosis, as already I have shown elsewhere, the deterioration is proportional to the schizophrenic component or coloring and a probable little more besides.

Our admirable and beloved and always interesting companions in this life, our misnamed " 'poor' relations", the brutes, the "speechless ones", have not as yet been studied much in regard

to their comparative behavior under sundry conditions. We know little of their mental derangements, therefore. I am convinced, however, that there is at times a deterioration of their intellects, particularly of the senile sort, comparable to that of their "relatives" who can speak. One sees this, naturally, most often in dogs (one of man's most precious friends), for example when his heart is failing and his brain is inadequately nourished. Many a noble dog has been killed who ought to have been sent to a sanitarium instead!

I think we are justified fully in accepting the doctrine that the cortex cerebri consists of layers parallel to the surface of the hemispheres and that these are functionally of two groups. The deeper of these two is the archepallium or "old covering", the other the neopallium or "new covering". The former has to do with actuating the more ancient activities mental and physical,—instinctive, automatic, and socially habitual. The latter, the neopallium, stands for civilization and for the culture of the individual. These latter, of course, all recognize as at heart habits of restraint, self-control, inhibition. Thus A. W. Campbell and J. S. Bolton have done more greatly than probably they at first realized when they gave us the key to the cortex cerebri. The infragranular layer does things of a basally habitual and instinctive nature. The supragranular layer now prevents it from doing too much, thus raising the individual from his archaic levels in the living world's evolution. *No other theory fits the facts.*

Among other important things, this relationship of the two great cortical layers in all probability gives us in its cytopenia the neuropathology of intellectual deterioration. The corresponding pathology for the affective aspect of mental regression, alas for our psychobiology!, is far less known, buried as it still is in the unsolved mazes of the autonomic and its relations to the brain. Of the dozen or more psychological factors in intellectual deterioration, at least half are intimately concerned with the archepallium.

As we have been learning so firmly of late, psychosis is a social situation in an individual, and a person living alone on an isolated island would find it difficult to meet the diagnosis. Deterioration, then, means a sort of atavistic reversion to a less inhibited and more primitive type of behavior and of life, and we have justification aplenty for postulating in regression a kind of descent of behavior-control, so to say, from the neopallium to the cortical archepallium and below. This is made all the more scientifically acceptable by the well-established fact that the neopallic nerve-

cells, last to develop both in the race and in the individual, are the first to deteriorate in severe emotional strains, such, for example, as terror and extreme nerve-fatigue. In passing, we may observe that these deteriorative changes mostly are permanent, and that this fact in a way corroborates our preference for leaving deliria and confusion, etc., out of the list of true psychoses, which are characterized by deterioration.

Deterioration is of importance in determining the question of competency and of incompetency on a basis more exact and more scientific than the usual estimation and opinion still usually employed. Psychometry is being used extensively now in the determination of congenital incompetency due to amentia. On the other hand, low intelligence from mental disease or from cerebral trauma, etc., in relation to incompetency still remains an indefinite matter of supposition on the part of persons who are seldom in the habit of making sure by modern methods of relative precision. Because of this the concept incompetency many times now means little because it is so vague. Worse than that even, it means different things in different places and for different purposes. For example, in one large organization a person often has been declared competent for certain purposes and incompetent for other purposes! And state laws disagree.

Most determinations of incompetency still rest on some individual's judgment, and there are few criteria of intelligence in a casual observation of a person unless he is at or near the imbecilic level. The symptoms of law intelligence are much less readily ascertained than are those of most psychoses. Unprogressive psychiatrists even estimate them at times with great uncertainty. For example, only a few weeks since I had from a psychiatrist a written opinion that a patient "looked about 10 years younger than his age, and has a childish appearance and is somewhat childish in his talk and manner. Examiner does not make out, however, that he is mentally deficient. He answers a few test-questions fairly well." (Shades of the psychiatry of the day-before-yesterday! Determining intelligence by the few test-questions which is all the average psychiatrist still knows of mental measuring!) On psychological examination this patient was found to have 6 as his base year; a mental age of 8 years, 3 months; an I.Q. of 51. He probably completed the second grade at the age of fifteen, having been left back five or six times. He has a feebleminded brother, and says that his sisters were much smarter in school than was he. He can read only the short words, and the

intrinsic qualitative evidence in the Binet-Terman scale is strongly corroborative. His age is forty-five.

Now, I venture to suggest when a professional psychiatrist has as little insight as this into one's intelligence, that in legal practice, in courts, in social service, etc., to judge incompetency by superficial indications that may be accidental or fortuitous or both, is a needless relic of the past. And for a certainty it is harder usually to judge a deterioration than a congenital deficiency of intelligence. And so we arrive once more at the inevitable conclusion that the determination and the measurement of intellectual deterioration is a matter of much practical as well as of theoretic importance in medico-legal work. Here it is an especially indispensable criterion of psychosis for a very special purpose. Every court-house needs a psychological psychiatrist *who doesn't have to guess.*

A competent psychologist familiar with the rudiments of psychiatry certainly can be taught or even teach himself or herself to determine and to measure deterioration of the intelligence in a reasonably short time. This has been proved for some years now. Far simpler and more convincing too are these deterioration examinations than is generally realized by most of you. This determination in a scientific way is of particular value and need in deciding the numerous borderline cases. In discussing these in court and elsewhere a large part of the unseemly contradiction in expert testimony has been almost scandalously given.

Again, with the ever greater acceptance of the continuity-idea between psychoneuroses and psychoses (stressed so convincingly, *e.g.*, by Kempf in his "Psychopathology"), the need of a ready psychotic criterion becomes ever more indispensable. For, after all, diagnoses in nosologic-type terms of some sort always will be useful for practical denotative purposes in Medicine and in Law. Description too would be difficult without diagnoses, as would also discussion.

It is with mental disturbances, disorders, diseases, disasters much as it is with tumors in the more somatic aspect of an individual. The main question to be answered to the person most interested is as to the benignity or the malignity of the intrusion. Intellectual deterioration is the readily accessible key to the answering of this question,—the most scientific answer, the most reliable, and the most easily available.

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THE RELATIONSHIP BETWEEN PHYSICAL HABITUS AND STUTTERING

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I. INTRODUCTION

WRITERS tend to divide themselves up into several schools in regard to their thinking about the nature and management of stuttering. One group holds that stuttering is a "bad habit", an inadequate speech habit. Treatment consists of substituting desirable for undesirable speech reactions by applying the laws of learning which operate in the "breaking" or acquisition of any reaction.

Another group believes that a speech or thought block arises when verbal imagery or mental speech recoils from the stream of consciousness. Treatment emphasizes the stutterer's attending to mental speech, listening for the words and speaking them by thinking aloud.

A third group maintains that stuttering is a personality disorder characterized by the stutterer's tendency to react inadequately, through speech, to certain or all social situations. The speech disturbance is the most noticeable symptom of deeper emotional conflicts. In treatment the emphasis is on emotional adjustment within the individual and the altering of his environment where certain factors such as the attitudes and habits of parents, teachers, and others, make reasonable adjustments on his part difficult or impossible of attainment.

A fourth group clings to the psychoanalytical viewpoint. Stuttering is thought of as a psychoneurosis in which there is a stubborn, sterile type of repetition of oral material that indicates oral eroticism. The stutterer does not want to "spit out" the words. He has regressed from the anal to the oral level and given the mouth a sphincter innervation which seems to indicate that he wishes to retain the words as "objects", using the mouth as a sphincter. The treatment is by psychoanalysis.

A fifth group sees stuttering largely as a manifestation of a deep-seated neurophysiological (probably constitutional) disturb-

ance. Management involves all of the known aids for stabilizing the human organism.

In the present study we have chosen a small part of the big problem in regard to the nature of stuttering for investigation. We took our departure from the work of Kretschmer (4) and his followers. In general, these workers have found that the manic-depressives tend to fall most frequently into the pyknic body type and the schizophrenics into the leptosome, athletic, and dysplastic body types. The manic-depressives and the schizophrenics represent two extremes of personality development. In mood, the former may vary between the elated and depressed, and the latter are usually poor in affect; in temperament, the former are wavy, and the latter jerky; and in psychomotility, the former are adequate to stimuli, rounded, natural, smooth, and the latter inadequate to stimuli, restrained, lame, inhibited, stiff.

The pyknic habitus. Individuals presenting the pyknic habitus are short limbed, compact and rotund with a generally well nourished physique. The bones tend to be delicate, the musculature soft and the fatty tissues abundant. The head, chest and abdomen are large in circumference and the shoulders narrow and the neck short. The face is soft, broad and round. The skull vertex is flat and the facial features are well formed. The facial shape approaches the five point or shield type. The hands are short, broad and soft. The hair of the head tends to softness and thinness, and there is usually early baldness, whereas the hair of the face and the body is quite profuse. The skin is usually smooth and of ruddy color.

The pyknoid habitus. The presence of marked pyknic components in conjunction with characteristics indicative of other physical types has been recognized by Wertheimer (5) who proposed the pyknoid habitus which we have incorporated in this study.

The leptosome habitus. Individuals presenting the leptosome habitus are characterized by a generalized deficiency in volume or thickness without any corresponding decrease in height. The thorax is long, narrow and flat and the shoulders narrow. The limbs and neck are long. The bones, muscles and skin are thin and delicate. The head is small and tends to be either high or round. The nose is usually long and with the small receding chin the profile displays a marked angularity. The shape of the face approaches the short egg form. The skin is usually pale. The asthenic is an extreme form of the leptosome type.

The athletic habitus. Athletic individuals are recognized by

a strong skeletal development and a prominence of musculature. The broad shoulders, narrow hips and slender lower limbs give the impression of tapering from above downwards. The bones are coarse especially at the shoulders, girdle and ends of the limbs. The muscles are hard and prominent. The skin is elastic and there is little fat tissue. The neck is usually long and well muscled. The head is high with fairly prominent cheek bones and a strong chin. The shape of the face approaches the steep egg form, although there is in some cases an approach to the shield type.

The leptosome-athletic habitus. A mixture in which characteristics of both the athletic and leptosome groups are found to a marked degree in the same individual is quite ommon. In the present study individuals presenting such a mixture are classified in a separate group, the leptosome-athletic.

The dysplastic habitus. Individuals with the dysplastic body form present a number of eunuchoid characteristics such as pronounced length of extremities, broad hips or other feminine traits in men as well as masculine characteristics in women, hypoplastic features, dwarfism, disproportions between different parts of the body and unusual distribution of fat.

The possibility of morphological and psychopathological correlations determined by Kretschmer and his followers opens a field for investigation with a bearing upon many lines of thought. In this particular study we were concerned with the question of how much a certain personality (physically and mentally considered) may be the proper soil for the development of stuttering.

II. SELECTION OF CASES

Forty-seven male stutterers ranging in age from fifteen to thirty years and 128 male normal speakers ranging in age from seventeen to thirty-seven years were selected for study. The mean age of the stutterers was twenty-one and seven-tenths, and that of the normal speakers was nineteen and seven-tenths years. Thirty-one stutterers were obtained from the Speech Clinic of the University of Iowa, and 16 stutterers from the Speech Clinic of the University of Minnesota.* A great majority of the stutterers began to stutter around or before school age. The normal speakers were selected at random mainly from freshmen and sophomore university swimming classes in the University of

* The writers are indebted to Dr. Bryng Bryngelson, Director of the Speech Clinic at the University of Minnesota, for his splendid coöperation in supplying certain cases and facilities for the purposes of this study.

Iowa. Both the stutterers and the normal speakers were representative of the middle west from the standpoint of nationality and residence.

III. PROCEDURES

The classification of the subjects according to body types was made on the basis of general observation and description of body form which took into account the nature of the following traits:

1. Relationship between shoulders, hips and chest
2. Face (shape and adiposity)
3. Eyes (setting and distance apart)
4. Mouth (size)
5. Lips (size and color)
6. Cheek bones (prominence)
7. Chin (breadth and protrusion)
8. Teeth (size and regularity)
9. Posture
10. Neck (length)
11. Genitalia (size, scrotum tone, irregularities)
12. Toes (length and thickness in relation to size of foot and body)
13. Fingers (length and thickness in relation to palm and body)
14. Feet (length and size in relation to body)
15. Hands (length and size in relation to body)
16. Moisture (degree, localization and distribution)
17. Veins
18. Pulse rate
19. Hair distribution and development (head, brow, secondary)
20. Skin (color and texture)
21. Fat (quantity and distribution)
22. Skeleton (bones and joints)
23. Musculature (prominence and hardness)
24. Relation of trunk to limbs

Each trait was evaluated according to the following scale: markedly below average, below average, average, above average and markedly above average. The face shape was expressed as short egg, steep egg, broad shield or flat five point, alloys being indicated through a combination of terms.

Weight was obtained in kilograms and height and chest circumference in centimeters. From these measurements the Pignet index was obtained for each subject according to the formula, Pignet index = height — (weight + chest circumference).

A classification of the stutterers according to mental types was made by evaluating the following traits as they were manifested by each case:

1. Good mixer
2. Cheerful
3. Moody (length and amount of external factors involved)
4. Flighty
5. Tenacious
6. Stubborn
7. Outbursts of temper
8. Self-centered
9. Shy
10. Asocial
11. Holds deep grudges
12. Suspicious
13. Industrious
14. "Nervous"

IV. DATA

Table I presents the classification of the subjects according to physical forms. It is to be noted that a much larger percentage of the stutterers than of the normal speakers fell into the leptosome and leptosome-athletic groups and that a much larger percentage of the normal speakers than of the stutterers fell into the athletic and dysplastic groups. Seventy-six and five-tenths per cent of the stutterers as compared with 24.2 per cent of the normal speakers fell into the leptosome and leptosome-athletic groups.

TABLE I

CLASSIFICATION OF SUBJECTS ACCORDING TO PHYSICAL FORMS

Body Type	Number of Cases		Percentage of Entire Group	
	Stutterers	Normals	Stutterers	Normals
Leptosome (asthenic)	23	11	48.6	8.7
Leptosome-athletic	13	20	27.9	15.5
Athletic	4	48	8.6	37.5
Pyknic		8		6.2
Pyknoid	4	9	8.6	7.1
Dysplastic	3	32	6.3	25.0
Total	47	128	100	100

The difference between these two percentages is 52.3±4.8. Thirty-seven and five-tenths per cent of the normal speakers as compared with 8.6 per cent of the stutterers fell into the athletic group. The difference between these two percentages is 28.9±4.0. Twenty-five per cent of the normal speakers as compared with 6.3 per cent of the stutterers fell into the dysplastic group. The difference between these two percentages is 18.7±3.5. All of these differences are statistically significant. Both normal speakers and stutterers were approximately equally represented in the pyknoid group. Not a single stutterer was diagnosed as a

pyknic. Because the pyknic habitus is most clearly defined in the individual of thirty years of age or more, it is entirely possible that with increasing age more of our subjects, especially the athletes, would develop a more easily observed pyknic body form.

The Pignet indices (Table II) range from the highest plus values for the leptosome group to the highest minus values for the pyknic group. This is the usual finding. In no case was there

TABLE II

PIGNET INDICES FOR THE VARIOUS PHYSICAL FORM GROUPS

	Stutterers		Normals	
	Mean	Range	Mean	Range
Leptosome	29.74	22.01 to 41.45	30.38	21.2 to 41.2
Leptosome-athletic	17.69	13.50 to 23.80	22.5	7.0 to 33.4
Athletic	15.58	12.28 to 20.00	11.65	—0.8 to 23.2
Pyknic			—13.03	—33.7 to 1.1
Pyknoid	5.81	0.0 to 16.50	—1.42	—28.2 to 18.9
Dysplastic	13.59	9.00 to 18.80	20.3	—5.1 to 47.5

a significant discrepancy with regard to any given individual between the Pignet index value and the physical habitus group to which the individual was assigned on the basis of observation and description.

For the stuttering group, Table III bears out the relationships commonly noted between mental and physical types. The great majority of the leptosomes were introverted and a great majority of the pyknoids were extroverted. Approximately half

TABLE III

DIAGNOSED MENTAL TYPES FOR EACH MORPHOLOGICAL GROUP (STUTTERERS)

	Introvert		Extrovert		Introvert		Ambivert Ambi.		Extro.	
	N	%	N	%	N	%	N	%	N	%
Leptosome	17	74.0	3	13.0	1	4.3			2	8.7
Leptosome-athletic	7	53.0			4	38.0	1	4.5	1	4.5
Athletic	2	50.0			1	25.0	1	25.0		
Dysplastic	2	66.7	1	33.3						
Pyknoid			3	75.0					1	25.0

of the leptosome-athletics and of the athletics were introverted, and the other half of each of these groups ambiverted. Two-thirds of the dysplastics were introverted.

For comparison with both our stutterers and normal speakers we have included in Table IV Gruhle's (2) distribution of body types among normal individuals. All of his subjects were males. The average age was twenty-nine and eight-tenths years. His

material was unselected both geographically and socially. The grouping of his subjects offers a striking contrast to that of our stutterers and a fair agreement with that of our normal speakers. With the exception of the asthenic-athletic group, his subjects were well represented in all of the groups.

TABLE IV

GRUHLE'S DISTRIBUTION OF BODY TYPES IN A NORMAL (GERMAN) POPULATION

Body Type	Number of Cases	Percentage of Entire Group
Asthenic	20	16.9
Asthenic-athletic	4	3.4
Athletic	22	18.6
Pyknic	15	12.7
Dysplastic	13	11.0
Mixed	26	22.0 (21.2% were pure or mixed pyknics)
Uncharacterized (normal proportions)	18	15.3
Total	118	99.9

V. DISCUSSION

The principal finding of this study is that stutterers as contrasted with normal speakers tend to fall into a single major physical group, the leptosome. There were relatively very few stutterers who presented mixed body types. This appears to be a suggestive finding. If Kretschmer and his followers are right, then not only the stutterer's personality but also his stuttering may be constitutionally determined. Acquired causative factors cannot be definitely ruled out. However, it appears that if they operate, they do so in connection with constitutionally predisposing conditions.

Our findings have a bearing upon the problem of the relationship between stuttering and personality traits. Fletcher (1) holds that stuttering is caused by social morbidity, hyper-sensitivity and timidity. Johnson (3) believes that stuttering, at least in part, causes these social reactions. As far as our study goes, it indicates that stuttering develops mainly in a person with a certain physical and mental makeup. Such may be taken to indicate that a particular physical and mental endowment may predispose an individual toward stuttering, and the views that the emotional reactions noted by Fletcher and Johnson are either causes or results of the stuttering should be supplemented with the concept that they are probably in part representative of constitutional factors.

How the leptosome habitus may predispose an individual toward stuttering is unknown. Considering speech as a kinetic expression of the personality, the leptosome individual with his psychomotor limp, stiffness and discordance might have difficulty in verbal expression. Speech, as one behavior characteristic of the individual, is paradigmatic of broader all-inclusive organismic characteristics. Each characteristic is a part and parcel of the organism itself.

Why all asthenically inclined individuals do not develop stuttering is probably due to the fact that other necessary hereditary and environmental provocative factors are not present. In this connection it would be enlightening to study the speech abilities and disabilities of leptosomes in the population at large. It is possible that they express speech disabilities in various degrees and in other than stuttering forms. Another interesting problem would concern itself with a study of the physical types in the family stock of the stutterer to see if the stutterer's ancestral stock is heavily laden with the leptosome habitus as well as stuttering.

VI. SUMMARY

1. By some of the methods described by Kretschmer and his followers we have made a study of the physical types presented by 47 unselected adult male stutterers and 128 unselected adult male normal speakers.

2. A majority of our stutterers as compared with a minority of our and Gruhle's normals fell into the leptosome and leptosome-athletic groups.

3. A minority of our stutterers as compared to a majority of our and Gruhle's normals fell into the athletic, pyknic, dysplastic, mixed and uncharacterized groups.

4. The great majority of our stutterers showed introvertive tendencies.

5. Because of the striking relationship discovered here between stuttering and physical habitus it seems reasonable to assume that constitutional determinants play an important rôle in stuttering. However, it is well to supplement this statement with the qualification that not all persons who are in this basic respect capable of stuttering need develop it. Whether or not they will develop stuttering depends upon further injuriously shaping, formative and predisposing influences.

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THE GESTALT COMPLETION TEST AND MENTAL DISORDER

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THE possibility that the Gestalt Completion Test * might be a measure of a very specific capacity involved in the perceptual process suggests its use as an aid in the clinical diagnosis of mental patients, the assumption being that this particular phase of the perceptual capacity might be disturbed by the illness of the patient. An effort has been made in this study to determine the reactions of mentally ill patients on the test, no effort being made to classify cases other than in the two general categories of organic and functional. The organic cases were known to have general paresis and in the so-called functional cases no gross physical findings could account for the illness, although the cases included psychoneurosis as well as psychosis. The sampling consisted of 263 patients at the Psychiatric Institute and Hospital, New York City. Forty-five of these patients were definitely diagnosed as general paresis, and 218 were considered as being functional cases. Of the functional cases, 110 were children and 108 adults, cases being classified as children if between the ages of nine and eighteen years.

The mean score obtained by the children was 6.32 and by the adults 6.35; as this difference was so slight the cases were grouped, the mean score of which was $6.33 \pm .087$, range 2 through 11. The mean score for a random sampling as reported in the original study was $7.38 \pm .049$, the difference of these two means being $1.05 \pm .10$. For the organic group the mean score obtained was $5.27 \pm .217$, range 0 through 9. The difference of the two means 7.38 and 5.21 being $2.11 \pm .222$. In both of these groups the differences are sufficiently great to be quite significant and would seem to indicate that patients suffering from mental disorder are not able to do as well on the test as normals, this difference being particularly marked in the cases of general paresis.

As no reports of test results for other than children have been given, it seemed wise to administer the test to a group of adults as a check. Ninety third year medical students obtained a mean

* Street, Roy F. A Gestalt Completion Test, Teachers College, Columbia University Contributions to Education, # 481, 1931.

score of 7.6±.113. The difference of this mean and the original mean of 7.38±.049 being .22±.123 which can hardly be considered as statistically significant.

CONCLUSION

The results of this study show definite inferiority of functional cases and marked inferiority of organic cases in the capacity to succeed on the Gestalt Completion test. The fact that the scores are inferior gives no indication, however, as to the exact nature of the deficiency. It might be due to a cloudiness of the perceptions or it might be due to actual regression or deterioration. As no effort was made in this study to record types of responses, further research might be revealing. There seems to be a tendency for younger children to juxtapose elements of the picture rather than to attempt to synthesize into a whole. If it were found that the inferior scores of the mentally ill were the result of a greater tendency to juxtaposition than to synthesis it would suggest regression rather than mental clouding.

THE DEGREES OF ACCEPTANCE OF DOGMATIC STATEMENTS AND PREFERENCES FOR THEIR SUPPOSED MAKERS [1, 2]

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THERE is a steadily increasing body of experimental data which indicates that opinions and preferences can be modified by a number of social factors. The work of Jenness [3] in 1932 on "The Rôle of Discussion in Changing Opinion Regarding a Matter of Fact" may be taken as an example of one possible type of experimental approach. As a somewhat different type might be mentioned two studies in which the junior author of this article was interested. In 1929 Farnsworth and Beaumont [4] demonstrated that the preference ratings of pictures can be influenced to a considerable extent by means of attached and suitably composed descriptive paragraphs. In 1931 Farnsworth and Misumi [5] published a study which indicated that artists' names have potencies in determining the preference values of pictures. That is, when it was supposed by the rater that a given scene or portrait had been painted by a well known painter it was rated as more beautiful than when it was assumed to have been created by an unknown painter.

It was to follow up this study in a different field that the present experiments were undertaken. The new problem was to ascertain the degree of acceptance of dogmatic statements under three different situations. One group of raters read a given statement with the impression that a certain well liked person had made it. A second group of readers was intentionally misled into attributing the statement to a certain disliked person. For the third group of readers the statement was attributed to no one.

The first problem consisted in obtaining a list of well liked and disliked persons. Such lists, of course, are strictly relative to the

[1] Reported in part before the 1932 meeting of the Western Psychological Association.

[2] Under the guidance of the junior author a similar study was planned but left in its initial stages by a graduate student, Charles Norman, in 1928.

[3] This Journal, 1932, 27, 279–296.

[4] Suggestion in Pictures. J. Gener. Psychol., 1929, 2, 362–366.

[5] Further Data on Suggestion in Pictures. Am. J. Psychol., 1931, 43, 632.

groups studied. Preferences probably vary to some extent from year to year, from district to district and from one to another of the social and economic levels. However, as is shown below, it was easily demonstrated that comparable groups in the same college react similarly. It should be noted that the aim was not to study the slight differences in preference [1] from one position to the next along the scale, but rather to obtain two groups sufficiently separated to justify the designation, 'well liked' and 'disliked'.

During 1930, 82 names were given ratings by 2 groups of raters—1 of students in elementary sociology (N, 102), and another from a class in introduction to education (N, 75). The raw scores which made up the 2 orders of preference intercorrelated at .96±.01 (product moment r). These same groups agreed to the extent of .79±.08 in their selection of the 10 best liked people and .93±.03 in their selection of the 10 least liked. Their combined data were employed in the final selection of liked and disliked groups.

The directions were as follows: "Read carefully. The following list of names is of people almost universally known. Some of them you like, some you dislike. You may have a real reason for your attitude, or you may not. No matter, we want your first reaction. Circle one of the letters following each name and indicate your feeling toward it. 'L' signifies liking or approval; 'D' means dislike or disapproval; 'I' stands for indifference. If the name is unknown, leave it unmarked." The votes for each person were forced into a single score by the use of the formula $100\frac{L+\frac{1}{2}I}{L+I+D}$. The 9 names which received the highest values, 'Twain', 'Jordan', 'Edison', 'Rogers', 'Einstein', 'Keller', 'Darwin', 'Aristotle', 'Lindbergh', were termed the 9 best liked names, and were employed in the subsequent experiment. The name 'Omar Khayyam', the tenth in the list, was omitted by a considerable number of raters so was not employed further. The name, 'Woodrow Wilson', actually the eleventh in the list, was finally chosen to complete the list of 10 best liked. As seven of the least liked seemed to be known by all the raters they were accepted as typical of this category. The names 'Julian', 'Guinan' and 'Tweed' were not very well known, and were replaced by 'Fall', 'Sunday' and 'Vallee'. 'Glyn' was also excluded because of the

[1] The terms 'preference', 'dislike' and 'like' are defined by ballot,—not necessarily by introspection. That is, a high score (a large V in the table) is arbitrarily defined as 'high preference'.

number of subjects who were unfamiliar with her. Preferences for the twenty names of the final list yielded unimodal distributions when a third group of 150 from a course in elementary psychology rated them (and the remaining 62) on a 5 point scale. This procedure was considered necessary as all too frequently in rating work one finds bimodal groups listed by their means,—a spurious procedure. The raw scores given by the psychology group correlated .98±.01 with the scores obtained from the other two combined groups whose values are reported in the table below.

TABLE 1

	R	V
Jane Addams	18	77.39
Roscoe "Fatty" Arbuckle	74	21.24
Aristotle	8	87.87
Benedict Arnold	57	45.23
Prince Bismarck	32	66.51
Napoleon Bonaparte	19	76.68
Clara Bow	77	18.51
Arthur Brisbane	51	48.89
William Jennings Bryan	42	57.45
Aaron Burr	67	32.95
Smedley D. Butler	36	63.93
Bishop Cannon	64	39.06
Al "Scarface" Capone	79	14.04
Anthony Comstock	58	44.96
Calvin Coolidge	29	67.16
Clarence Darrow	30.5	66.59
Charles Darwin	7	88.08
Eugene V. Debs	48	50.53
Jack Dempsey	46	51.38
Thomas A. Edison	3	94.94
Albert Einstein	5	90.55
Albert B. Fall	72	24.77
Henry Ford	33	65.40
David Lloyd George	12	83.47
Mahatma Ghandi	16	78.42
Elinor Glyn	70	28.32
Emma Goldman	62	41.38
William Green	41	60.51
Texas Guinan	75	20.23
Warren G. Harding	55	45.99
Will Hays	47	50.91
William Randolph Hearst	80	12.45
Sen. Thomas Heflin	68	29.95
Gen. Von Hindenburg	23	74.79
Herbert Hoover	15	78.45
Rupert Hughes	52	46.55
Robert G. Ingersoll	30.5	66.59
Hiram Johnson	66	37.98
David Starr Jordan	2	95.57
C. C. Julian	76	19.64
Helen Keller	6	89.01
Omar Khayyam	10	84.08
Nickolai Lenin	50	49.54
Sinclair Lewis	37	63.73
Charles A. Lindbergh	9	87.39
Judge Ben Lindsey	34	64.26

TABLE 1—Continued

	R	V
Walter Lippmann	17	78.20
Aimee Semple McPherson	82	6.41
Queen Marie of Roumania	63	41.12
Karl Marx	39	61.43
Andrew W. Mellon	21.5	75.42
H. L. Mencken	40	60.71
Mohammed	28	69.73
Tom Mooney	59	44.17
J. Pierpont Morgan	35	64.10
Benito Mussolini	49	50.00
George Nathan	27	70.06
Thomas Paine	24	74.44
Alexander Pantages	78	14.53
Kemal Pasha	38	62.50
Pope Pius IX	65	38.53
John Raskob	44	52.28
Will Rogers	4	94.14
Margaret Sanger	25	71.47
Sun-Yat-Sen	26	71.39
George Bernard Shaw	20	76.57
Upton Sinclair	43	56.40
Al Smith	60	43.17
Joseph Stalin	45	51.54
Billy Sunday	71	26.02
"Big Bill" Thompson	81	12.22
Leon Trotsky	53	46.17
Gene Tunney	21.5	75.42
Mark Twain	1	95.59
W. M. "Boss" Tweed	73	24.34
Rudy Vallee	69	28.76
Pancho Villa	61	42.96
Voltaire	13	82.21
James "Jimmie" Walker	56	45.45
H. G. Wells	14	80.74
Geo. Wickersham	54	46.05
Woodrow Wilson	11	83.68

Note: An R (rank) of 1 indicates 'best liked'.

The construction of the questionnaire employed in this study followed orthodox lines. On the first page were the usual spaces for name, sex, and age. The directions were as follows:

"In these days of discontent there is so much talk and expression of opinion that it is difficult to know just what the thinking people are thinking. This is a survey to find how students of American colleges stand on current social, economic, and political problems. The following are typical clippings from recent periodicals. Indicate, by circling the appropriate number in the margin, the degree with which you agree or disagree with each statement.

The meaning of the numbers is as follows:

① Absolute and complete agreement.
② Partial agreement.
③ Undecided; not ready to express an opinion.
④ Partial disagreement.
⑤ Absolute and complete disagreement.

Now turn the page and work rapidly, but do not fail to circle one figure in each line."

A typical sample taken from the 30 items of the test would be:

"1 2 3 4 5 There is nothing sacred about the American constitution. If it doesn't serve its purpose, it should be changed as often as necessary. (Wilson) (Big Bill Thompson) (——)"

The names in the parentheses indicate that one group of raters saw Wilson's name at the end, one saw Thompson's, and one saw no name.[1] Care was taken to employ names consistent with the contents of the items. As will be shown later, several failures were apparently recorded.

The major data of the experiment can be seen in Tables 2, 3, and 4.[2]

TABLE 2

	Name	D/σ_D 1.2	1.3	2.3	N	Mean
1	Jordan				159	2.60
2	Blank	5.7	6.5	.9	150	3.44
3	Arbuckle			.	152	3.57
1	Wilson				161	1.67
2	Blank	1.5	5.4	3.8	152	1.89
3	Capone				150	2.49
1	Lindbergh				152	1.97
2	Blank	4.7	5.1	.2	149	2.69
3	Vallee				160	2.72
1	Edison				160	3.53
2	Blank	3.1	4.5	1.2	152	4.00
3	Bow				150	4.16
1	Darwin				152	1.79
2	Blank	.9	4.8	3.5	150	1.92
3	Sunday				158	2.41
1	Wilson				149	2.40
2	Blank	2.2	3.3	1.1	152	2.75
3	Capone				158	2.94
1	Wilson				151	2.05
2	Blank	.5	3.8	3.3	150	2.13
3	McPherson				161	2.66
1	Lindbergh				160	2.58
2	Blank	2.8	3.2	.4	152	3.01
3	Thompson				149	3.08

[1] This combination of names did not occur. Because of the libelous character of the experiment the names actually used are not linked with the item offered as the sample.

[2] The subjects who served in these experiments were from classes in elementary sociology, education and citizenship.

TABLE 2—Continued

	Name	D/σ_D 1.2	1.3	2.3	N	Mean
1	Jordan				152	1.88
2	Blank	2.0	3.4	1.4	160	2.15
3	Vallee				149	2.36
1	Rogers				160	2.98
2	Blank	2.5	3.3	.7	152	3.34
3	Pantages				150	3.45
1	Rogers				150	3.18
2	Blank	1.8	2.5	.6	152	3.49
3	Hearst				160	3.60
1	Lindbergh				150	1.78
2	Blank	1.4	2.8	1.3	161	1.97
3	Hearst				152	2.16
1	Edison				150	2.86
2	Blank	2.1	2.5	.6	160	3.15
3	Sunday				152	3.23
1	Darwin				150	3.40
2	Blank	2.1	2.3	.1	161	3.71
3	Bow				152	3.73

TABLE 3

	Name	D/σ_D 1.2	1.3	2.3	N	Mean
1	Aristotle				150	2.25
2	Pantages	4.6	5.1	.2	152	2.88
3	Blank				158	2.96
1	Aristotle				152	3.47
2	Arbuckle	1.8	3.3	1.4	160	3.76
3	Blank				150	3.98
1	Jordan				150	2.98
2	Thompson	.3	2.6	2.3	152	3.04
3	Blank				160	3.40
1	Darwin				161	2.90
2	Hearst	2.2	2.5	.2	150	3.20
3	Blank				152	3.24
1	Einstein				160	2.81
2	Fall	.6	1.8	1.1	149	2.91
3	Blank				152	3.08
1	Edison				151	3.59
2	Pantages	.8	1.8	1.0	160	3.71
3	Blank				150	3.85
1	Keller				152	4.22
2	Sunday	.1	2.0	1.6	150	4.24
3	Blank				159	4.46
1	Twain				152	4.10
2	McPherson	1.2	1.6	.2	149	4.27
3	Blank				160	4.30

The heading '1.2' refers to the D/σ_D value between the first two items of each section under 'Name', the '1.3' to that existing between the first and third, and the '2.3' to that between the second

and third. For example, in the first comparison the '1.2' value is 5.7. This means that the acceptance of a certain dogmatic statement was significantly greater ($D/_{\sigma D}$, 5.7) when it was attributed to Jordan than when attributed to no one. The name of Jordan was found to be more potent than that of Arbuckle ($D/_{\sigma D}$ between '1.3', 6.5) in eliciting acceptance of this same statement. There was not a significant difference in acceptance ($D/_{\sigma D}$ between '2.3', .9) between the situation which no name was present and that in which Arbuckle's name was seen. In general it would seem that the data of Table 2 indicate that the readiness to accept dogmatic statements varies to some extent with the likeableness of the persons to whom the statements are attributed, being greater when a well liked name is attached, and least when a disliked name is seen. Although the $D/_{\sigma D}$ values are by no means all significant, the trend would appear to be in this direction.

In Table 3 there are presented instances in which the order of acceptance is: greatest when the well liked name is attached, next greatest when the disliked name is seen, and least when no name is present. The reason for the variation of these results from those of Table 2 is not clear to the authors. The $D/_{\sigma D}$ values of Table 3 do not approach significance as frequently as do those of Table 2.

TABLE 4

	Name	$D/_{\sigma D}$ 1.2	1.3	2.3	N	Mean
1	Blank				152	3.96
2	Twain	0	.2	.2	150	3.96
3	Bow				160	4.00
1	Capone				152	2.37
2	Einstein	.3	.7	.3	150	2.42
3	Blank				160	2.48
1	Blank				149	3.95
2	Rogers	.4	1.7	1.3	152	4.01
3	Thompson				160	4.18
1	Blank				150	3.47
2	Vallee	.1	.2	.05	152	3.50
3	Aristotle				159	3.51
1	Blank				150	3.48
2	McPherson	.2	1.1	.8	151	3.52
3	Keller				161	3.67
1	Blank				152	2.71
2	Arbuckle	.8	1.5	.5	150	2.85
3	Twain				160	2.93
1	Fall				152	2.95
2	Blank	.4	.4	0	160	3.02
3	Keller				150	3.02
1	Fall				158	2.91
2	Blank	1.1	3.7	2.6	150	3.08
3	Einstein				152	3.44

There remained 8 statements which had the greatest acceptance either in the situations in which no names were attached, or in those in which the disliked names were present. In only 1 instance did the $D/_{\sigma D}$ value approach significance, the explanation for this value seeming to lie in the notion that Fall certainly had more practical acquaintance with the subject matter of the statement (American politics) than had Einstein. Other somewhat similar explanations could, perhaps, be developed for the remaining 7 variants.

It seems possible to conclude from the present study that, in general, dogmatic statements are more likely to have greater verbal acceptance when they are attributed to well liked personages than in the situations in which they are attributed to disliked people.

EXTRA-SENSORY PERCEPTION OF THE CLAIRVOYANT TYPE

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I WISH to make here a brief report of 37,377 tests in card-guessing made under conditions planned to exclude the known sensory functions from aiding in obtaining successes. The results of these tests have remarkably surpassed "chance" expectation and, under the conditions, stand as quite unexplainable by modern psychological theory. In such a situation there are roughly two modes of procedure, the one consisting in holding that such data are simply erroneous (as an *a priori* consideration), and the other, that of carefully extending, varying, and relating the data until their ultimate explanation is achieved,—until we do understand them. Because of its scientific immaturity, perhaps, academic psychology has throughout the past followed the former procedure, when confronted with such data, rejecting the facts of extra-sensory perception because they appeared unexplainable and mysterious.

It has not at all been for lack of carefully collected evidence that students of psychology have hesitated to take an inquiring attitude toward extra-sensory perception (telepathy, clairvoyance, etc.). During the half century and more that has passed since Professor Barrett [1] tried to bring the subject before the British Association for the Advancement of Science at Edinburgh more than a score of valuable evidential contributions [2] have been made, and no serious flaw has ever been shown to exist in the main bulk of this work. The only vigorous attempt made in this direction was that of the Lehmann hypothesis of "involuntary whispering",[3] in connection with telepathic experiments, and this had a short life in the hands of critical experts in the field.[4] Its inapplicability was very evident in connection with the better evi-

[1] Barrett, W. F., F.R.S.E., "On Some Phenomena Associated with Abnormal Conditions of Mind". Proc. Soc. Psy. Res., I, 238–44, 1883. Read before Brit. Assoc. in 1876.

[2] Most of these may be found reported or reviewed in the Proc. Soc. Psy. Res. (London).

[3] Hansen, F. C. C., and Lehmann, A., "Über Unwillkürliches Flüstern", 60 pp., 1895.

[4] Particularly see Prof. H. Sidgwick's reply, Proc. Soc. Psy. Res. (London), XII, 298–315, 1896.

dence [5] and this hypothesis was pushed no further even by its author himself. A definite point was made of its successful exclusion in many later experiments.[6]

Rather has this indifference of psychologists to the data of extra-sensory perception been due to the limitation so many of us have,—of requiring an understanding before investing credence. A fact has to be "squared with the known", to be shown to be "lawful", and related to the already recognized natural order, before it *is* a fact for most of us. This is only a proper safeguard for the scientifically naïve mind, since it prevents many missteps. Perhaps for a young branch of science such conservatism is required.

The absence, then, of laws, relationships, explanations, and the like, in the experimental evidence for telepathy and clairvoyance of the past has made it unacceptable. But once we can show its principles, laws, curves, and natural relationships, or if we can pronounce the magic words: "Anyhow, it works!" referring to some practical application, interest and belief from students of psychology will not be wanting.

On this very score it is believed that the present report may be of some interest to those who have hesitated hitherto. For it is fair, I think, to say that we have here a definite step in the direction of the "naturalization" of extra-sensory cognition, one that points to natural relationships that would seem to be well within the reach of quantitative study. The step we have been able to take is a short one, but the way is open for more. (Others indeed are well in progress in our laboratory.) This point is especially urged for those to whom such data have hitherto been "simply too impossible".

Very few will need to be reminded that, if extra-sensory perception occurs—in however small a way—the fact is of tremendous importance. Its bearing reaches far beyond the limits of psychology, to many other fields. Its challenge to physics alone will probably involve fundamental reconstruction—at least expansion,—since no kind of wave mechanics seems applicable. What it might mean to a scientific religion, if we had one, we can only vaguely infer; but every great religion has been based on a creed

[5] *E.g.*, Telepathy with agent and percipient in different rooms, Proc. Soc. Psy. Res., Vol. VIII, 536–96, 1892.

[6] To cite only two of the more recent studies, both made in psychological laboratories: Brugmanns, H. J. F. W., "L'etat passif d'un telepathe controle par le phenomene psychogalvanique", Compt. Rend. 2d Cong. Internatl. de Metapsychique, Warsaw, 1923.

Estabrooks, G. H., "A Contribution to Experimental Telepathy", Bull. V, Boston Soc. Psy. Res., 1927.

saturated with belief in these non-sensory modes of knowing and communing, and all theologies have been built in part from such beliefs. Again, there is the field of practical applications. Back of these strange phenomena doubtless lie larger principles that may be discovered, extended, and applied in the manner that has made the great achievements of science in the past. And for psychology alone the significance of extra-sensory perception extends far beyond our capacity for immediate comprehension. We can vaguely see that it may be crucially involved in the whole range of body-mind problems, and the ultimate reach of this connection could not even be conjectured. All problems and theories that relate to the ultimate nature of mental process will properly have to reckon with these facts of non-sensory perception and their implications,—and this is a very sweeping relationship. In the laboratory, too, there is always the disturbing question of possible error due to the telepathic and clairvoyant factors at work in the responses of the subjects. "Jungle of the mind?" Quite naturally, to those who are unacquainted with its large literature. And what, in point of fact, was *not* at one time "in the jungle"? If we can keep our perspective and know our scientific method, we have here a rich field of exploration. The frontiers of science are ever the borders of the jungle.

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This short report covers the results of tests made by my colleagues, assistants, and myself at Duke University during the two years 1931 and 1932. A fuller report will appear elsewhere,[7] filling in the great gaps that will necessarily appear in this more limited presentation, and reporting later results and conclusions from the experiments as they continue.

We began with a view to covering both telepathy and clairvoyance in our experimentation, but after a while decided to discontinue for a time the telepathic study. This has been made on hypnotized subjects, chosen at random from volunteers, and was the work of Dr. Helge Lundholm and myself (he having contributed his excellent technique in hypnosis). But the procedure was very time-consuming and proved to be but little more profitable in data of significance than was the simpler procedure which, at the same time, Dr. K. E. Zener and I were carrying out on another set of subjects with a view to testing for clairvoyance.

[7] This has appeared in the meantime as a book entitled "Extra-Sensory Perception", published by the Boston Society for Psychic Research, 719 Boylston St., Boston, Mass. The book covers three years' work and reports 90,000 trials, including work in both telepathy and clairvoyance.

The 1,115 telepathic trials were on the whole above chance expectation to a small extent, but in view of the several divisions with different chances for a single trial, their total significance would be difficult to compute. The largest division, 530 trials on number-guessing, ran above chance expectation to a deviation of only 2.6 times the probable error.

Another procedure which we began and discontinued—in this case Dr. Zener was author of the better part of the technique, and coöperated in the work—was that of making group or class tests, giving out sealed envelopes containing figures to be guessed "clairvoyantly", recorded, and returned unopened. Our 1,600 trials of this sort ran very little above the chance estimate, but they did reveal the presence of at least one phenomenal "guesser" in our classes. This procedure had been, like all our beginning methods, exploratory, and after finding it did not give us results of any particular interest, we gave it up. It, too, was laborious and slow.

If we subtract the 2,715 trials mentioned in the last two paragraphs above, the total of 37,377 trials is thus reduced to 34,662. No one will question, I think, the propriety of not including these 2,715 trials in the total computation, since they were the product of entirely different conditions and techniques. Failure under these different conditions means nothing damaging to success under others. Moreover, with but few exceptions, the subjects, too, were different. Inclusion of these 2,715 trials would, however, do no serious damage to any of the grand totals or conclusions of the rest of the work.[8] We turn, then, to what developed into the main experimental procedure, clairvoyant card-guessing by individual subjects made with a probability of 1/5 for each trial.

The subjects were, with but few exceptions, students at Duke University. At first we tried all who were willing, and entered all trials into the record. But after about 2,600 trials, we began the custom of giving a preliminary test to the new subject, and accepting him for trial for record only after he made a test score well above the chance expectation. The only inflexible feature of this test was that its results should not count for record, no matter how good or bad.

The simple experimental procedure consisted in having packs of 25 cards, containing five cards each of five stamped designs (star, circle, rectangle, wavy lines, and plus sign), shuffled and

[8] The book referred to in Note 7 gives full details of the results and conditions.

placed face down before the subject. He was then asked to call the top card, making a "pure guess" and avoiding all rational thought so far as he was able. He was permitted to raise the card up a little from the rest, or hold it off by itself. The observer was, of course, alert for any deception or violation of the method. During the first 1,500 trials, approximately, of the 34,662, after the subject would call the card and it was recorded, he would be allowed to turn it over at once for a check-up on correctness. But this necessitated too frequent change of cards to prevent any learning of the cards by slight markings on the back or edge and it was discontinued. The method thereafter was to have either five cards or the whole 25 called off and recorded and have these piled up in the order in which they were called. Then they would be checked for correctness by turning them all over at once and calling back down through the hand. This avoided the consecutive visual experience of first the back and then the face of the card, which would facilitate learning them from any possible back-markings.

Some of our best subjects, as well as some others, quite commonly do not look at the backs when calling the cards. Our plan has been to have many packs of cards, and to switch them frequently. No conditions for "stacking" cards are permitted. Tendencies of the more practised card-players to keep track of the numbers of each type or suit of card are discouraged, since this tends to lower the score. The cards were tested for opacity with 250 watt bulbs. None of our many witnesses, which include a magician, and three professional psychologists, have discovered any use of sensory perception in the tests or have pointed to any possibility of it explaining the results.

Some of our results,—not the best of them, however,—were obtained without any observer present. Three assistants, Mr. H. L. Frick, Mr. C. E. Stuart, and Mr. J. G. Pratt, have worked alone, as have also my sister-in-law and myself. The assistants are all graduate assistants in our department, and, of course, are fully trusted, or our relations would never have been entered into. But our most striking data are of my own witnessing, and a large part of these was taken in the presence of two of the investigators or with an additional observer. Furthermore, no conclusion would be altered by omitting all the unwitnessed trials. There is no good reason for us to exclude them, however; on the other hand, they offer an independent check on some of the peculiar relationships we shall later point out. They total about 14,000 of the

34,662. Had they been our highest results the case would, of course, be different.

Of the 34,662 trials made on extra-sensory card-perception with individual subjects, 7,583 trials must be held for special description later on in this report, since they were made with special conditions designed definitely to deal with some particular phase of the general problem and which might alter the score. The remaining 27,079 trials, then, form the main block of the regular experiment. Of these, 12,010 trials were made under the direction of Mr. J. G. Pratt upon himself and others; 7,500 were made by Mr. C. E. Stuart; 4,715 were personally witnessed by myself.

It is important first to note just what we would expect to get from 27,079 trials, if there were no extra-sensory perception. The probability for each trial is 1/5, since there are 5 suits or types, and the average chance expectation would be the number of trials (27,079) times the probability per trial (1/5), or "np". This (np) would be 5,416. And this calculated chance expectation works out very closely in practice as well. In 4,000 trials made to test this,—trials in which extra-sensory perception was excluded by making random calls first without cards and later on, matching shuffled cards against them,—the number of hits was almost exactly the np value; it was 801 as against 800 for np. For 1,000 of these check trials, I took that number of the actual calls previously made for record by our best subject and matched the cards against them from the same packs, re-shuffled. The result was 204 hits, as against 200 for chance or np. This is only 2 per cent away from the prediction, and is, of course, negligible.

The *actual* yield of successes for the 27,079 trials was 6,759, or as many as *1,343 above the chance expectation.* This deviation from the np value is 25 per cent; that is, the successes are 125 per cent of chance or np. But what is much more significant, for our evaluation of them, in view of the large number of trials involved,—this difference of 1,343 is more than *30 times the probable error*[9] for np for this number of trials. This is a figure so significant mathematically as to leave no doubt of excluding chance as an explanation for these results. The odds against getting such a result by mere chance are so many billions of billions to one that there is simply no point in making the computation. Explanation by chance is ruled out.

To all who appreciate the significance of these figures, it is abundantly clear that we have something at work here of a defi-

[9] *I.e.*, probable error, computed as $.67449 \sqrt{npq}$, with n = number of trials, p the probability of successes with each trial, and q the probability of failure.

nite super-probability character—a factor that helps in the successful calling of these cards. If we have been successful in our exclusion of normal sensory perception, we have, then, evidence of a mode of perception beyond the normal sensory kind. The only question remaining, therefore, is whether we actually have been able to exclude the normal sense perception, as we tried definitely to do. Since this is the only question, let us keep it in mind as we briefly survey something of the distribution of the data given above as totals. This is as striking as the significance of the totals itself.

The results of two subjects stand out above the rest. These are Mr. Hubert Pearce and Mr. A. J. Linzmayer. Mr. Pearce came to our attention through my hearing that his mother and other relatives had possessed "psychic ability" (which, however, had not been publicly demonstrated nor commercially exploited). He was a student in the Duke School of Religion and is now a licensed minister. His results in their totality are the best we have. Mr. A. J. Linzmayer was discovered by the class-tests mentioned above. He was our first subject of major importance but he now ranks second to Mr. Pearce on the totals. His mother, too, has had what are called "psychic experiences", notably premonitions and monitions.

Mr. L. is, like Mr. Pearce, a young man of very good ethical standards. I have tried him out under the influence of narcotic drugs (on the principle *"In vino, veritas"*) and found no will to deceive. But neither of these men has ever been given an opportunity to deceive us; both have been under observation by at least one investigator at all times during the testing. Cards were changed so frequently as to exclude either normal perception of marks on the back from playing a part or any hyperesthesia of minute features unnoticeable to the investigators. In 2,250 trials by Mr. Pearce, for example, more than twelve packs of cards were used. The first runs of these cards yield results as high as second and later runs. He commonly does not look at the cards. In Mr. L.'s case, too, the best scores were made without his seeing the backs of the cards, or even touching them. These, and further conditions described under special experiments below, well exclude all possibility of deception, either conscious or unconscious. The results from these two men up to July 1, 1932, are given a tabular presentation for purposes of easy comparison.

It will be noted from the last column that the results from these two men, and those of Pearce alone, appear to be more significant than that of the group of subjects as a whole, including them.

This is not strictly correctly estimated, since it supposes in the computation for the grand total that the higher scoring of the two men may theoretically be distributed anywhere over the whole group of 27,079 trials, which is not true. But we need not make the calculation for the higher value, since anything over 30 times the probable error is so enormously improbable as "chance" that it could hardly be any more impressive to the judgment than that figure itself.

There are many special points of interest in the results from these two men, Mr. Pearce and Mr. Linzmayer. First, they both

TABLE 1

COMPARISON OF RESULTS OF PEARCE AND LINZMAYER, WITH GRAND TOTALS (UP TO JULY 1, 1932)

Subject	No. of Trials	No. of Hits	Deviation	p.e.	Value of X [10] (Dev./p.e.)	Hits Per 25
H. Pearce	2,250	860	+419	±12.8	32.7	9.6
Linzmayer	2,649	791	+261	±13.9	18.8	7.5
Total	4,899	1,651	+680	±18.9	36.0	8.4
Grand Total (75 subjects)	27,079	6,759	+1,343	±44.4	30.2	6.2
Chances	4,000	801	+1		0.0	5.0

ran, at their best, about double the chance expectation: Mr. P. is still at his best; Mr. L. declined very sharply, after 600 trials. His results were still significant, but much less so. I will speak of this later. Mr. L. had flashes and slumps during his high series. Mr. P. moves along at a more even rate. L. ran straight series of successes, twice with 9 consecutive hits, and once with 15. On this last occasion he got all correct in 25 except 4, whereas P. has never gotten over 6 straight successes,[11] and never over 15 hits in 25, though he has several times done that. (The odds against one getting, as he did, 15 consecutive successes by chance are 30 billion to one; and of getting 21 hits in 25 distributed in any way, are 100 billion to one. The odds against L. getting the totals he did, even though there is a much lower percentage of hits, but with a large number of trials, runs very much higher than this.

[10] This column gives a relative value to a given set of trials, taking account of their number and their scores together. It is convertible by means of Probability Integral Tables into the odds against the scores being due to chance. The odds in this table are, however, so enormous as to be beyond our capacity to appreciate. In this column the gain over np. (*i.e.*, deviation, d) is divided by the probable error, p.e., and gives x. With $x = 30$ we have odds against chance that give a number with 90 digits. This is a safe approximation, and is so unquestionable a figure as to dismiss chance as a competing hypothesis.

[11] Since writing this Mr. P. has gotten longer runs of straight successes, one of 25, without a break. His value, d/p.e., too, has risen to well over 60.

Other peculiarities will be mentioned farther on under the special experiments.

Among the other subjects are some, too, who have reached notable values. Mr. C. E. Stuart is one of these. His work with himself as subject, consisting of 7,500 trials, yields 1,815 hits as against 1,500, a positive deviation from chance expectation of 21 per cent or 13.5 times the p.e. Here again we have results that exclude the chance explanation from further consideration. This work also will be referred to later in more detail. (It was mainly unwitnessed; though occasionally I had Stuart perform before me as a partial check on the whole series. During these 140 trials his score was 23 per cent above chance expectation, slightly above his average for the 7,500. As mentioned above, he is a graduate assistant in our department.)

The table below will give the data of the main experiment arranged according to the observer responsible and will supply the basis for judging the relative value of the various divisions. The results obtained in each of the first three groups are very strikingly significant, and they represent relatively independent work. Only two subjects were shared by the observers. The small "Miscellaneous" group consists of data witnessed by Dr. Zener, Mr. A. E. Lecrone, and 1,000 unwitnessed trials by Miss Miriam Weckesser, my sister-in-law, all included for completeness. The test of "pure chance", the 4,000 trials made without any possibility of extra-sensory perception operating, is also included in the table. Then the special tests are included, just to let the reader see them in terms of totals and significance. It will be seen that they do not in any way hurt the scoring record, even as merely so many more trials; but of course their special value

TABLE 2

TABLE OF ALL DATA FROM CLAIRVOYANT CARD-GUESSING (TO 7-1-32) BY INDIVIDUALS UNDER WAKING CONDITIONS WITH PROBABILITY OF 1/5 PER TRIAL

Observer — Group and Conditions	No. of Trials	No. of Hits	Deviation and p.e.	X or d/p.e.	Hits Per 25
General Experiment:					
J. B. Rhine—40 subjects	4,715	1,356	413±18.5	22.3	7.2
J. G. Pratt—30 subjects	12,010	2,910	508±29.6	17.2	6.1
C. E. Stuart—1 subject	7,500	1,815	315±23.4	13.4	6.1
Miscellaneous—4 subjects	2,854	678	107±14.4	7.4	5.9
Total, general experiment	27,079	6,759	1,343±44.4	30.2	6.2
Special:					
J. B. Rhine—For low rec.	1,875	308	67±11.7	5.7	4.1
J. B. Rhine—Other spec.	2,588	619	101±13.8	7.3	6.0
H. L. Frick—Spec. experiment	3,120	829	49±16.3	3.0	(p=¼)
J. B. Rhine—"Pure Chance"	4,000	801	1	0.0	5.0

lies, not in their total quantitative relation, but in what they show or suggest about the nature of the process of this mode of perception.

Before going into the special experiments let us raise the question again, "Have we been deceived, and are our data unreliable?" For us, who know these scores of friends, assistants, and students who have served as subjects, the question is a more or less academic one. And it is, perhaps, far-fetched for anyone to suppose them all guilty, consciously or unconsciously, or both. But in any case the question may properly be centered upon the work of the best two subjects. Mr. Pearce and Mr. Linzmayer were well observed throughout. Mr. Pearce ran most of his trials with two observers. Various conditions were laid down, too, to give special safeguards. Linzmayer's best results were obtained without vision or contact with the cards; many of Pearce's without vision of them. Involuntary whispering does not, of course, come into consideration, if no one else knows what the card is, as was the case in all but a few hundred of these trials.

But let us come to closer grips with the question of deception, once and for all. Two of us, Mr. Pratt and myself, gave Mr. Pearce six *new packs* of cards, and watched him call them all three times over with his usual average score of approximately double the chance expectation, or 100 per cent above np. He seldom fixed his attention on the backs of the cards, and he did not touch the faces. (For that matter, the cards are stamped lightly with rubber stamps and fountain pen ink, which do not leave impressions sensible to touch.) We think it fully safe to say, deception under such conditions is excluded by the conditions.

Still one more advance, however, and I think no one will hold out for the deception theory. We placed a shuffled pack of cards before Mr. Pearce; he closed his eyes and, without touching the cards at all,—leaving the solid pack unbroken—he called down through the pack, naming the twenty-five cards as he "guessed" them. They were then checked for successes. This was carried out, in all, eleven times, making a total of 275 trials. As compared to chance expectation of 55, he obtained 87 successes, which is 32 points, or 58 per cent, above np. What is more important, it represents a deviation of more than 7 times [12] the probable error of np, a figure which no one will hesitate to regard as significant.

[12] This represents odds against mere chance of approximately 100,000 to 1. At the date of mailing of this manuscript this series had been expanded with Mr. P. alone to 1,625 trials, with a deviation over *14 times* the probable error, giving odds against chance of about 1,000,000,000,000 to one.

Of course, it still remains to suppose that Mr. Pratt and I, as well as all the assistants and subjects, are all rogues; one of us alone would not be enough! We would all have to be "in cahoots"!

. . . . . .

And now, finally, for a brief sketch of some of the interesting natural relations of extra-sensory perception, as they have been suggested by special tests and by special features in the general experiment. These relationships are pointed out as interesting suggestions—not as conclusions. From accepting the general fact of extra-sensory perception itself I see no intelligent way to escape; but these reflections made from our special tests, which are as yet fewer in number, are not so clearly forced upon us by logical necessity. As it is, only a part of these experiments can be included here, and these very briefly; but the larger report will attempt to amplify all points left incomplete. Our chief interest now is to show the beginning of the "naturalization" or "demystification" of this process.

First, what does a narcotic drug do to this mode of perception? Does it increase its function, or decrease it? And how much, compared with sensory perception? With Mr. Linzmayer, a large dose of sodium amytal, 15 grains, inhibited for the time of its effect, all extra-sensory perceptive ability. In 275 trials made in this condition, he got only 56 correct, only one point above the chance expectation. Before and after the test period he was running at a level of 35 per cent above np. This drop of 33 per cent and the subsequent return to it indicates a decided depressing effect of the drug upon this perceptual function.

Mr. Pearce was given a much lighter dose, only 6 grains, but allowing for body weight differences the dose was equivalent roughly to about half that given Linzmayer. While Linzmayer became considerably incoherent in his speech, Pearce remained distinctly rational. Both were very sleepy, but whereas L. was unable to keep awake of his own volition during 275 trials, P. was able to remain awake during 325, and appeared able to go on further, had not other circumstances made it inconvenient for us to continue. Under the influence of the drug, however, Pearce too dropped from his fairly uniform level of 100 per cent above chance average to 22 per cent above during the 325 trials under drug influence. At first he dropped to a chance level exactly, and held there during the first 75 (he made only 12 hits instead of his usual 30 or thereabouts). Then he made a great effort to "pull himself together", became more alert, and made 10 hits in the

next 25. Again he relapsed into drowsiness for several 25's, and ran about chance; after washing his face in cold water, however, he rose to a 6 and an 8 in 25, and encouraged by this, he boldly asserted that he could "read right down through the pack" without touching the cards,—as he had done several times in the normal state. He followed up this assertion with an 8 and a 10 in the next two series of 25 each (and did this without touching the pack). All the rest are chance or below. Mr. P. seemed to have enough self-control to be able to recover his ability when he made a great effort to do so, but this was difficult and he tended to "dissociate" again at once. It is important to add that neither subject knew what to expect from the drug, and it was not therefore thought necessary to disguise it. Both tried to score high, as usual.

From these few but striking data it appears, first, that extra-sensory perception of this type is easiy interfered with by the narcotic drug, sodium amytal, and roughly in the degree to which it affects the higher mental processes in general. It suggests that this mode of perception, too, is a function of the nervous system, at least in some vital part of its action. Secondly, it seems to suggest in the data of Pearce particularly, what we will have more evidence to show below, that the volition and degree of effort of the subject are able to control this mental function, very much as they control any other mode of perception, though not as completely. A third feature of interest is that sensory perception remains after extra-sensory disappears, as far as our few data are representative. That is, it would suggest that extra-sensory perception requires more perfect organization or "integration".[13] (This could, of course, only be applied to the condition produced by this particular drug.) On the McDougall theory of mental levels as affected by narcotic drugs, this further suggests that this mode of perception may stand on a higher level of complexity than the sensory. These are suggestions only, but no one would hold that they are entirely groundless; and they serve to point up further experiments.

Next come the tests that involve volitional control, since they connect up in interest with the drug effects. It was shown above that Pearce could rise in scoring, while under the influence of the narcotic drug, by making great effort to recover his normal alertness. This is, however, merely indication of voluntary control of

[13] Dr. W. F. Prince suggested that the narcotic results tend also to "block the suspicion that perhaps after all, sensory perception was somehow employed in the experiments without drugs."

degree of attention, and of the fact that better attention in this case helped the extra-sensory perceptual process. But in this connection there is an interesting problem which was pointed out by Miss Jephson [14] in her study of clairvoyance, particularly with reference to the telepathic studies of Richet [15] and of Estabrooks.[16] She mentioned the fact that in both studies there had been a drop below chance, under adverse conditions; in the one case when the series was made too long; in the second, when a repetition series was called for and the subjects had to wait over for it.

But it would appear theoretically that mere fatigue could not account for a "sub-probability" curve; fatigue of the process itself ought to yield a "chance" approximation, and in the light of our drug experiment I think so even more strongly. This sub-probability, it seemed, could be explained only by a definite purposive factor, quite like the "super-probability" or extra-sensory cognition factor, itself,—simply reversed. This explanation would work well enough if under the circumstances extra-sensory perception were turned to work against the desire of the experimenter. If the subject is pressed into service beyond his own desire and becomes rebellious, he may, while superficially desiring to be courteous about it, seek to express his distaste for the task by calling the cards in some careless or whimsical manner, really avoiding or departing from the usual way, and thus *avoiding successes*. This is more likely to be the case "unconsciously" since no one knows just how he does the guessing anyhow. The degree of clear realization of just what he is doing, as he calls the cards under silent protest from within, may vary a great deal. Some may be convinced they are doing their best, and yet in their ignorance of the process and desire to escape the strain, may actually be thwarting it and making it work against the score.

With the hypothesis in mind as just described, I forced Mr. Linzmayer through 1,650 trials, under conditions calculated to make him rebel. He is very courteous and coöperative, and can be made to work when he does not want to. These results were committed to "Low Record Tests" beforehand, and have been kept apart from the main experiment. All such tests are labeled before and never after the results are obtained. These 1,650 trials yielded 291 hits, as against 330 for chance expectation. This is

[14] Jephson, Miss Ina, "Evidence for Clairvoyance in Card-Guessing", Proc. Soc. Psy. Res. (London), XXXVIII, 223, 71, 1928.

[15] Richet, Charles, "La Suggestion Mentale et la Calcul des Probabilities", Rev. Phil., XVIII, 1884.

[16] Estabrooks, G. H., "A Contribution to Experimental Telepathy", Bull. V, Boston Soc. Psy. Res., 1927.

12 per cent below np, and nearly 4 times the p.e. These results based on so large a number of trials offered some support for the hypothesis, since in the tests I deliberately tried to work against L.'s desire so as to induce mild rebellion and turn his effort against the success of the work. (It should be pointed out that Mr. L. is somewhat negativistic; this is revealed in hypnotic experiments, drug tests, and personal questioning.)

It seemed to be a proper next step, then, to try to get a subject to run low deliberately, quite as he would consciously or unconsciously do if he were pressed into service against his desire. We asked Mr. Pearce to score as near to zero as he could, and he seemed fully willing to attempt this. He had just scored an 11 and a 13 in the last two runs of 25 previously called. At once he dropped to 1 in 25, the first time he had ever done this, and throughout the 225 trials made for Low Record, he ran an average of less than 2 in 25, and 62 per cent below the np value, which gives us a drop of 162 per cent of np, and the impressive figure of 18 times the p.e. for the np. It was abundantly plain that P. could direct his ability whichever way we asked him to. In fact we had him alternate in series of 25's, and he obtained for high record 9; for low, 1; for high, 10; for low 1 again.[17] Again, I think this favors the hypothesis given above, of the explanation of the results that seem to follow too adverse conditions. We are dealing with a process that is dirigible by normal volition. And in any case this seems to relate it more definitely to the recognized processes of mind; perhaps all the more so, if it does, as I suspect, sometimes follow the subject's conation without his full awareness. Such so-called unconscious compulsions and inhibitions are found to be common enough, and on another score, then, we might welcome this extra-sensory mode of perception to the fold of psychological relationships,—to "naturalized citizenship".

And now we come to curves! But we cannot do justice to them all here. There are the operating curves, the so-called "fatigue curves", and ability-decline curves, that seem to stand out most plainly, and these will serve our introductory purpose here. First, then, the operating curves, illustrated in the case of a particular subject. Let us present Mr. Pearce's "curve" as our best case of regular operation of this mode of perception, and of its

[17] Since this report was written, Mr. C. E. Stuart has resumed his experiments with marked success, and has tried making 2 calls for each card, one for a "hit", and one for a "miss". He has called in all 1,300 trials for low record, and has gotten only 70 per cent of chance—30 per cent below. At the same time the 1,300 calls for high score ran over 30 per cent above chance. This is highly significant, and confirms Pearce's work.

orderly functioning within the mental life of the subject. Needless to say, this curve may not fit anyone else; it is Mr. Pearce's individual way of working under the conditions as they now are. He, too, may change in time.

Mr. Pearce has made a large number of his trials (1,375) by calling off the cards from the pack of 25, laying them down in hands of five, and after each five, having the observer check for correctness. This has given a rhythmic effect in the results, in runs of fives, and we find that we get five similar curves within the run of 25, if we superimpose the scores of all the runs and total the hits for each ordinal from 1 to 25. This shows that P. tends to repeat the same curve with every hand of five he lays down, and that for our purposes, we might as well regard the run as in fives instead of 25's. The totals of all the columns from the first trial to the 25th runs as follows, with 11 for chance expectation substracted from each: 16, 18, 10, 1, 8; 13, 17, 8, 9, 7; 18, 23, 7, 7, 8; 12, 21, 12, 6, 4; 21, 23, 9, 4, 0. And when these are totaled as to consecutive trials, we get the following yield above the np value: 80, 102, 46, 27, 27. This general curve of frequency of hits seems to be typical of the way Mr. P. works. Even if we divide his data into still smaller lots and total them, we find the same consistent way of working. He gets predominantly most on the second trial, next in number on the first, and this closely followed by the third; in some small sub-groups, he got more in the third than in the first. The last two are almost regularly lowest. On this basis P. works rather uniformly, his last five of the 25's coming out about the same as the first. Out of these 1,375 trials P. got 557, or 282 above np. This is 28 times the probable error. (Taken from general experiment.)

With more space in a larger report other curves of operation will be discussed. For example, Mr. Pearce has a different one when he calls down through the deck without a break. But the one instance here presented serves to show the "naturalness" of the factor responsible, revealing its orderly expression under routine conditions. This brings it within the limits of the systematic activities of the mental life of the organism, even though we are far from understanding just *how* it works. It seems clearly to be a part of the normal mental make-up of Mr. Pearce, however it may have become so. This is the principal point made here.

Next, the "fatigue curve". (I do not like this name, since it

is not known that fatigue is the responsible factor,[18] and it would not be clear just which fatigue was meant anyhow—general fatigue, or fatigue of the extra-sensory process involved. I think Miss Jephson first used the expression, doubtless in a tentative way, suggesting a possible analogy.) The type of curve which I shall mention under this heading is not, it seems quite probable, a result of fatigue, at least not directly. But we will use the name in quotation marks, until we get a better one.

Mr. H. L. Frick set out to ascertain what kind of a curve he would get, if any, by running 100 guesses on playing-cards daily, himself the subject. He had previously had some success in getting above chance in tests of clairvoyance, but had worked mainly with drawings.[19] He did this on his own initiative, and the factor of unconscious antagonism ought not, therefore, to be expected. After 9 days he drew up the curve plotted by successive totals of the 20's and got a fairly good curve of decline of frequency of hits with the successive twenties of the hundred. The totals of successive twenties ran: 58, 50, 48, 38, 36. This is not much above np for the whole series, but the first 40 per day and the last 40 are fairly significant in their own right, and especially significant by contrast. Np is 45 for each total of 20's, and with this subtracted we have +13, +5, +3, −7, −9. Mr. Frick was encouraged to continue, but at least two new factors were then present: The large factor of the subject's intensified expectations (earlier he had not known what to expect). And second, a much interrupted schedule. Often more than 100 were run on the same day. The first 900 should stand alone, therefore, but in order to let the reader judge for himself the rest are fully reported, except 320 from interrupted series which are only included in the total in the longer table. After 600 more trials, 1,500 in all, the result above chance expectation stood as follows: +25, +9, +10, −10, −2. It still makes a strong curve of decline, but its former smoothness is gone. The last 1,300 were scattered in distribution, as I think we might fairly have expected from the strained conditions, and yet the curve is not completely ruined in spite of this: +26, +6, +19, −4, −5. But, I repeat, the more proper part of the test lay in the first 900. We should not expect in such delicate exploration as this to measure a man and find him the same after showing him

[18] Fatiguing of extra-sensory perception seems unlikely with the relatively small number of trials per day; more recently Stuart ran 700 trials for *high* score and 600 calls for *low* scoring, all in one day. These results are the best he has ever done—1,300 trials in all.

[19] Frick, H. L., "Extra-Sensory Cognition", M. A. Thesis, Duke University Library, 1931.

his results and letting him know what we expect of him. We have our Heisenberg Principle in psychology, too; where it has long been a commonplace practical handicap.

One more case of "fatigue decline curve"; we have not made efforts to gather special data on this point, but a special test given Mr. Linzmayer by Miss Helen Turner of Navesink, N. J., using 50 sealed envelopes supplied by me, reveals an excellent decline curve, though the results are themselves not above chance expectation. They consist of six series of 50 trials each, and when totaled as to tens in the order of running, they stand as follows: 19, 13, 15, 9, 4. Or when np is subtracted: +7, +1, +3, −3, −8. This makes a pretty well balanced figure, with its extremes significantly widely separated; that is, +7 and −8 are both between 3 and 4 times the probable error of the np; and their difference, which interests us particularly, is over 5 times the p.e. of the difference. This series of 300 trials, then, has a vulnerable point, even though its low end cancels its good beginning.

How shall we explain these "fatigue curves"? Surely these men did not rebel at doing this work. Both volunteered to do it. This calls for a different explanation than the "low record" cases produced by pushing subjects against their will. Briefly, I propose this hypothesis for trial and guidance in our further study: Since the subject (of the class of Frick and Linzmayer) does not know how he gets a card correct by extra-sensory means,—does not know the "feel" of such perception—and cannot guide himself intelligently, but rather has to grope vaguely after it, the natural result of *long effort and strain* would be to fix more and more upon some habit or routine way, and to depart from the openness or relaxation required for groping one's way to success. He might under strain come to distrust the very rudimentary method that yields success, and not know it; distrusting and thus avoiding the actual experience, unwittingly, he would the more systematically call the card erroneously. Thus he would come to lean more heavily upon an "hallucinatory" experience of a card than upon the actual extra-sensory perception of it. We have such effects, sometimes from extreme strain and fatigue, from drug effects, and other disintegrative causes, in the case of common sensory perception. In this mode of perception (extra-sensory) then, indicated already by our drug experiments to be more sensitive to modification than sensory perception, the strain of prolonged exercise might well lead to its being hallucinated. This would systematically turn it to work against its former higher score, and

run it below chance expectation just as the visually hallucinated person may systematically avoid an object actually perceived. But again, whether or not this hypothesis is found to be acceptable eventually, would be highly debatable now; one thing, however, is scarcely debatable, and that is, that these curves show still further order and relationship to exist between this extra-sensory process of perception and the process-system of the organism as a whole. Again we have stepped beyond the mere evidence that *it is*, that it *occurs*,—and we are finding that in point after point, it is somehow linked up, intricately related, and naturally a part of the functional complex of mental life.

There is another species of decline curve. This is the decline of ability with the course of time, often ending with the total loss of capacity. This has been reported in the literature of the subject several times. The case of the Creery girls [20] come first to mind. The subject, Van Dam,[21] used by the Groningen psychologists, Heymanns and Brugmanns, was another. These were reported as telepathic subjects. The telepathic horse, Lady,[22] was another instance, in a different species. Strain, overwork, and the strong urge to be right every time and to seek sensory or rational routes to knowledge, are commonly offered as explanations.

Mr. Linzmayer began with a high rate of scoring. He ran an average of 9.9 hits per 25 during the first period of 600 trials. (Most of these were made in a three-day period, followed by the running of 900 under chosen adverse conditions, committed for Low Record at the start, and already described above.) Several months later we resumed tests with Mr. L. and found that, under the very best conditions, he could not hold above 6.5 hits per 25 on an average for the 945 trials we obtained. Still later we ran 960 trials with him, with substantially the same rate, this time 6.7 per 25 trials.[23]

Mr. C. E. Stuart's record of his own clairvoyant calling of the cards serves as another instance of this decline. In this case the tests have been made more regularly, and with no disturbance

[20] See report in Sir William Barrett's book "Psychical Research", Home Univ. Library Series, 1911, London.

[21] See Brugmanns, footnote 5.

[22] Rhine, J. B., and Louisa E., "An Investigation of a 'Mind-Reading' Horse", Jour. Abn. and Soc. Psy., XXIII, 449, 66, 1929, and the sequel, "Second Report on Lady", same journal, Vol. XXIV, 289–92, 1929.

[23] Since the writing of this report Mr. L. has returned for more experiments, and in the 2,000 clairvoyant tests made he got 5.9 hits per 25. We have then for the four periods of testing with Mr. L., 9.9, 6.5, 6.7, and 5.9 as the order of decline in hits per 25.

caused by special experiments. He ran thus 7,500 calls, with 1,815 successes, a gain of 315 or 21 per cent above mean chance expectation. This deviation is 13.5 times the probable error. Now, if we make 5 divisions of these data, taking them by 1,500's, the yield above chance is: +128, +67, +43, +54, +24, which makes a very marked curve of decline. In percentages above chance expectation: 43 per cent, 22 per cent, 14 per cent, 18 per cent, 8 per cent. At last he discontinued for a time, believing it unprofitable to continue, since the last few series had been around np. But he has recently resumed, after several months' rest, and is back up, temporarily at least, to his highest level again.[24] Another of our subjects, Miss Miriam Weckesser, who held to an average of 6.3 hits per 25 for 1,000 trials, yielding a deviation 6.6 times the p.e., has also lost the ability, and cannot at present run profitably above chance, as shown by the preliminary test system in use. Mr. J. G. Pratt ran at a rate of 5.5 hits per 25 for 2,880 trials, yielding a deviation 3.9 times the probable error, and then dropped off, also. We have had no others who have produced for a short time and ceased. Mr. Pearce alone has shown no sign of a decline.[25]

I do not intend to try to offer a suggestion as to the explanation of this decline at present. But I point out again that it adds a further sign of relationship in still another direction, and again one that can be quantitatively studied. Whether it will be found that these curves (in general) are governed by motivational peculiarities, by the exhaustion of some function that is essential to extra-sensory perception, or by complex changes of the mental system as a whole, such as strain or distort or suppress this delicate process into the characteristic curve, in any case we will have to deal with interrelations of this extra-sensory function with the other processes of the psychic organization. We cannot escape this outcome.

In a word, then, we have to face not only the fact of the actual existence of extra-sensory perception: we have to accept it as another normal process along with the rest—simply less understood than some. The drug effects, the evidence of its control and regulation by voluntary effort, and the various curves described in the above paragraphs showing peculiar order, and

[24] But he has, since the above writing, already gone through his ability-decline again. By 400's, the 1,600 trials he has since made for high scoring run in excess over chance (or np) as follows: 43, 30, 29, 9; and 1,200 trials for low scoring, in deviation below chance by 300's: 23, 20, 19, 13.

[25] Since writing this six other good subjects have been discovered who have not shown serious decline as yet, though they have each made several thousand trials.

causal relation, all testify to natural interrelationships between this mode of perception and the rest of the mental activity of the organism. And if such phenomena are indeed natural to the organism,—a functional outgrowth of its organization,—then not only may we regard it as respectable psychology to study them,—indeed we must greatly expand and qualify our existing concepts of the physical and psychical nature of the human system in order to explain them.

. . . . . .

This introductory piece of work bristles with interesting sub-problems, and these seem very rich in possibilities. If adequate financial aid could be obtained many of these could be carried out within a short time. Trained assistants can be secured, and subjects can be obtained. We have many other half-completed or merely-started bits of experimentation. We wish, of course, to work out the telepathic phase of extra-sensory cognition, separate it clearly from the clairvoyant, or discover its relationship to the latter. In all earlier work on telepathy there is, I believe, the possibility of confusion of the two. We want to try the effect of caffein, and a range of drugs, on those who can score significantly. We want to work toward isolation and control of the special conditions affecting degree of this capacity, and toward this we have some promising leads but no certainties.[26]

I cannot give too much prominence to the fact that without the unusual tolerance, courage, and breadth of perspective of Professor William McDougall, Head of our Department of Psychology, we could not so openly conduct this work in a psychological laboratory. It will be recalled that he fostered the study of telepathy at Harvard by Estabrooks. I emphasize this sponsorship because in the history of psychology it will surely deserve to stand out when these contributions, themselves, are forgotten.

I have to thank Duke University for a special research grant.

The assistance and open-minded attitude of my colleagues, Dr. H. Lundholm and Dr. K. E. Zener, are especially appreciated. Their encouragement has meant a great deal in work of this kind. Mr. Charles E. Stuart, Jr., Mr. J. Gaither Pratt, and Mr. Harvey L. Frick have given me valuable help and I acknowledge their large share in the work reported here. It is one of my chief regrets that I dare not advise these able young men to continue to do research in this promising field of psychological study in

[26] Already there are promising results on caffein, on the effect of strange visitors on initial results, on the effect of illness, of changes in technique, etc.

which they have acquired interest and experience—simply because of the well-grounded fear we have that it might hurt their future in academic psychology. This is the perilous inbreeding principle of orthodoxy, one of the worst impediments of science.

General Note Appended June, 1934

This article was written in the summer of 1932 and in the interests of conservatism I let it lie on my desk until the summer of 1933, when I submitted it to this JOURNAL. Meanwhile the work advanced rapidly and it seemed necessary to insert certain points indicating more recent progress. These were made as footnotes, in order to leave the original report stand as written.

During the year of delay in publication by the JOURNAL the report of all work up to the summer of 1933 has been published (see note 7); this includes and greatly amplifies the data of this article and offers in addition as strong a case for the telepathic phase of Extra-Sensory Perception as this brief report does for the clairvoyant. This account will, however, I hope, help to bring the work of this laboratory to the attention of interested psychologists, whose field is obviously concerned.

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"INSTINCT" AS AN EXPLANATORY CONCEPT*

By LESTER S. KING, M.D.

IN 1843 Oliver Wendell Holmes brought forward the view that puerperal sepsis, the dreaded fever that so frequently followed childbirth, was of the nature of an infection. This aroused considerable opposition. One obstetrician of the time, Meigs by name, declared "he preferred to consider the disease as due to the workings of Providence, which he could understand, rather than to an unknown infection of which he could form no conception".(1)

This response, by which a single word is used as if it were an adequate explanation, seems naïve. The same naïvete has been attributed by some to the word "instinct" in psychology. Thus, one excellent text book of psychology ridicules the word as follows: "Instinct, like many another traditional notion, became an explanatory concept. Why did a child cry when hungry? It was because of an instinct to cry. Why did one fight when cornered . . .? Because of an instinct to fight"(2) and so on. Indeed, "instinct" in psychology has even been compared to the "virtus dormativus" as a pharmacological explanation.

However, the word should not be dismissed too lightly. In the interest of scientific method and of theoretical psychology it would be well to consider its meanings and implications.

"Instinct" has had many meanings. It is not necessary to make a survey of the history and development of the term, for much has already been written on that subject. But a brief survey would bring up the following points in reference to behavior: (1) Instinct is usually defined as a tendency, impulse, drive, disposition, or faculty—all of which mean essentially the same thing, namely, an active force, the nature of which is unknown. (2) This force—let us arbitrarily call it "disposition"—is innate and inherited. (3) This disposition is concerned with purposes, ends or goals.

An able discussion of instinct from the behavioristic viewpoint (with which we shall tentatively ally ourselves) is furnished by Tolman (3). For him instinct is defined in terms of drives or sets which are innate and permanent. The goal is some par-

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ticular type of rearrangement of organism and external object. For any given drive the initiating stimulus may be either intra-organic or external. In the service of these drives are "secondary responses", such as trial, search, preparation, which are capable of adaptive adjustment to given external conditions, to fulfill the aim of the drive.

On the other hand, an avowed opponent of the concept of instinct builds up a scheme of behavior which is rather similar. Kuo (4) starts with "units of reaction", which are random movements, such as sneezing, yawning, movements of the head, of the trunk, of the arms and legs, sensitivity to sound, light, and temperature, and "hundreds of others". These various movements are entirely random. They do not serve biological ends, nor are they at all teleological or adaptive. Behavior as such is integrated out of the random movements, that is, such movements combine into a "reaction-system". The particular reaction system (integrated response—L. S. K.) elicited by a given stimulus depends on the "behavior set"—which is the general disposition making the organism respond in one way rather than another. This is not a driving force, impulse, or active agency, but only a disposition facilitating certain reactions, inhibiting others. It is because of this "behavior set" that a hungry child seeks food instead of gurgling. However, such behavior sets are all the result of experience and are not innate; and this is his point of difference from any impulsion or hormic psychology.

In other words, movements random within admitted limits, but in themselves non-adaptive, happen to fall together into a satisfactory combination, which then becomes impressed on the organism and helps to determine behavior in the face of the next similar stimuli.

These two views may be contrasted as follows: both sides are interested in stimulus, response, and the intervening state of bodily organization. The opponents of instinct maintain either (a) that the response is random; or (b) it is determined by a condition or "set" resulting from past experience or present environmental stress. The proponents of instinct, on the other hand, maintain that what determines the particular bodily organization is some disposition independent of experience, innate, and propulsive. The two views agree that certain stimuli, either exteroceptive or interoceptive, act upon the organism and continue to act until some consummation releases the tension and the activity comes to an end; and that the effectiveness of stimuli is limited by the constitution of the organism.

With these latter two statements no one could possibly disagree, regardless of their attitude. It is equally obvious that the correct procedure for the advancement of knowledge is to investigate the specific forces which act on the organism, and the specific condition under which behavior takes place. Such researches have been very fruitful, and have resulted in such concepts as hormones, accessory food substances, maturation of the nervous system, and the like which are all directly relevant to behavior. Relating a given instance of behavior, such as maternal behavior to the action of vitamine E or manganese adds to our knowledge.

The concept of forces, in whatever terms they are expressed, acting on an organism, is an acceptable one. The task of biology is to define these forces as adequately as possible. The concept of "innateness" of forces is vague, possibly irrelevant, possibly without meaning. Hence at first view the doctrine of innate instincts, classified to the number of two, five or twenty-five, seems unnecessary, since any given "instinct" can be described in other terms, which are physiological or bio-chemical, or theoretically reducible to such terms. Given an objective, concrete (even if sometimes quite hypothetical) physical basis, what does the concept of instinct add? The behaviorist must assume that any given behavior is explicable in physico-chemical terms. Therefore why use "instinct" which by definition is only another word for "some force or other, the nature of which is obscure"? Before discussing these questions we have raised we must consider certain facts of scientific method.

II

At this point let me go back and consider poor Dr. Meigs, who preferred Providence to bacteriology in the explanation of puerperal sepsis. This is not quite as ridiculous as it sounds.

It is not necessary to labor the point that science aims at description in terms that are as general as possible. The laws of science, it is agreed, exhibit no compulsion, but are only shorthand notations expressing a relationship between observed events. The most general form for a scientific law is aRb, where *a* is one entity, event or concept, *b*, another, and *R* the relationship that holds between them. This is frequently expressed as a=f(b), but this form is of less generality. The explicit introduction of the concept of function implies mathematical expression. Mathematical functions are only a special case of Relations.

For example, Newton's law of gravitation, that any two particles attract each other with a force that is directly proportional

to the mass and inversely proportional to the square of the distance, is expressible as aRb, where a and b are any two particles, and R the relation between them, expressible mathematically as $\frac{Kmm^1}{d^2}$. This form holds good whether R be phrased in strictly mathematical form or not.

In this perfectly general scheme, two terms are related by some concept. "America is to the west of "Europe" is such a description on perhaps the simplest level, where "to the west of" is a simple relation, although the terms "Europe" and "America" are complex. However, the relation need not be simple. The formula for Newton's law of gravitation is somewhat more complex, although still perfectly definite. In mathematics, physics and astronomy, events, terms or entities are related by concepts which are usually exact, and varying in complexity from the equations of Einstein down to "square", "square root", or other simple functions.

But in the less exact sciences such as biology, sociology, anthropology and the like, functions relating the two given terms are more vague. They are phrased in words, rather than in purely conceptual notation. Yet even such vague concepts as "the action of adrenalin", or "the character of Bismarck" may serve as relations between given terms: *e.g.*, when a cat in a certain situation responds in a certain way, which we call fear, "action of adrenalin" may be the connecting link. Or two historical events in German history may be brought into relation with each other through the relating concept "character of Bismarck". So long as a concept has a definite meaning it may serve as the R in a description.

Between description and explanation there is a difference only of complexity. The formal schema aRb is identical in both, but the R is more complex in one than the other. For example, "a snake is more differentiated than an earthworm" is a simple description, where two terms are related by the simple concept of "differentiation". If we ask "Why?", and seek thereby an explanation, we relate the two terms "snake" and "earthworm" by the concept "Doctrine of evolution", thereby preserving the form aRb, but making the R very much more complex. The fact that evolution cannot be expressed in exact formulæ detracts no whit from its value as a relating concept, strictly analogous to Newton's formula. Formally there is no difference between

description and explanation, nor between exact and approximate expression.

But the value of a relating concept depends on more than formal perfection. Otherwise there would be no distinguishing a true from a false description, or a good from a bad explanation. Such value depends only on certain pragmatic criteria, provided that each concept has a definite meaning. Among these criteria I shall mention (1) Simplicity: Of two alternative relations (*i.e.*, descriptive or explanatory concepts) the simpler is the better, other things being equal; (2) Generality: the concept that applies to the greatest number of data (given individual terms) is preferable; (3) Adequacy: the application of a concept must not involve inconsistency, whatever data future research may produce.

Consequently, we see that Dr. Meigs in preferring Providence to bacteriology as explanation for puerperal sepsis committed only pragmatic error. Between the two terms "a well woman" and "a sick woman" he interposed the concept of Providence, which is definite enough and not inconsistent with the teachings of bacteriology. But it is not simple. To be adequate to the data at hand it is unnecessarily complex. Ultimately infection and resistance may be related to the Divine, but in the present state of our knowledge "we have no need for that hypothesis".

III

If now we return to "instinct" we can evaluate it more properly. Our problem now becomes, of what value is this term as an explanatory concept for a given instance of behavior? Here I would distinguish proximal from general explanation, which differ in definiteness.

The proximal explanation of an event should be in specific, concrete terms, and specific terms are reducible to material physiological data. A given stimulus (situation) and a given response must be correlated by means of the most concrete possible data. Where we have demonstrable entities, such as hormones, catalysts and nervous pathways, they must be preferable in proximate explanation, to vague, non-material concepts. As we have said before, the task of science is, in the first place, to gather as many concrete proximate explanations as possible. "Instinct" as generally used does not come under this category.

But the business of science includes more. Not only must data be collected, resumed, and correlated, but the correlations must proceed in an ascending scale of generality. Eventually very gen-

eral concepts will be reached which will be applicable to the entire field. In such a case any event will be completely describable by means of a single concept which will be complex but adequate.

In the present state of our knowledge such general concepts are necessarily less concrete, less mathematical, less rigorous than a specific proximal explanation. Since such general, widely applicable theories or concepts are a philosophic necessity, we create them out of the material at our command. In biology the problem becomes: What are the forces acting on organisms, and how is the totality of such forces most adequately expressed? Such a general concept is primarily applicable to the summation of all possible behavior, and derivatively to a single instance of behavior. There is thus a difference in exactness of expression between proximate and general explanation, but there may not be any inconsistency.

It is at this point that the concept of instinct becomes of value, not as a proximate but as a general explanation. It is only necessary to define what the concept means and implies, and what pragmatic value it has.

(1) Since "instinct" is of value only as a general and not a specific concept, the implied generality should be stressed. "Instinct" is a derivative term, and implies an impulsive or *hormic* biology. It is noteworthy that of the many psychologists who approve of "instinct", there is agreement concerning this general dynamic or hormic implication, while there is disagreement over the number and character of specific instincts. Consequently we may say that separate specific instincts are not valuable as explanations in psychology for they add nothing to the description of behavior. "Why does an organism do any particular thing? Because it has an instinct to do so"—is a type of description that has been quite adequately and justifiably ridiculed. But Instinctive or Hormic Psychology is valuable since it stresses a definite viewpoint.

(2) Hormic psychology, therefore, maintains as we have said in the beginning, that one of the forces acting on an organism is an innate inherited disposition concerned with ends or goals. As Monakow and Mourgue (5) phrase it, "All living beings have certain things in common, namely, a certain propulsive tendency, the tendency to approach and unite themselves with certain things or objects, or to retreat and defend themselves from certain others. They grow, adapt themselves to a common environment, reproduce, and maintain themselves in a certain state of security. To

the general propulsive tendency, which assures the individual the maximum security for the distant future as well as the immediate present, the name 'horme' is given (ὁρμη a setting into activity)" (6).

McDougall (7) states as the essence of hormic theory: "To the question—Why does a certain animal or man seek this or that goal? it replies: Because it is his nature to do so." He does not mean that such an answer is adequate explanation for any specific event; that would obviously be absurd. He means that organisms do act as a whole towards certain goals, with adaptation to circumstances. Such action owes little or nothing to prior experience. So too Herrick (8) speaks of instincts having a forward and backward reference. This means "the animal tends to react . . . in fixed modes that favor the preservation of the individual or the species . . . the organization has been so shaped by its past history (the backward reference) that it can do no other." This organization has been shaped by biological processes, not by mystic forces.

In other words, hormic psychology maintains that certain reactions are determined by innate forces driving the organism towards certain ends; that such forces are biological, not mystical, but that the present concepts of biology do not adequately describe such forces. Consequently the vague general term Instinct or Horme, must be kept.

(3) The unity of biology is stressed by this concept. Life is a continuum, and therefore some concepts must be applicable to the whole range. Vertebrates and invertebrates, organisms with and without a nervous system, all exhibit this Horme, the matrix of the so-called separate instincts.

(4) The concept of "Instinct" insists that appearances are real—behavior viewed "as if" it were purposeful, displays an appearance which is not to be neglected. This purposiveness is precisely the common thread which unites the whole of biology. Protozoölogy and psychiatry, neurology and embryology may all be united by this common concept. It is to Monakow and Mourgue (5) that we owe the beautiful and simple illustrations of this fact, and since I have discussed their work at length elsewhere (6) it is not necessary to dilate upon it here. Suffice to say that hormic psychology implies the unity of psychobiology. In other words, many separate disciplines both clinical and laboratory, and in fields apparently widely disparate, all exhibit a propulsive force inherent in the organism, that is useful in the

description of such sciences. Psychology becomes an intimate part of biology, a field in which no branch is sharply marked off from any other. Consequently the avoidance of narrow pedantry and the taking of all biology to be one's province, is implied by "Instinct".

(5) This concept insists that function is more important than form. The opponents of hormic biology must say: Given certain bodily characters (such as neurones, synapses, nervous organization, and the like, all innate) plus certain suppositions about say, pleasure and pain, all behavior can be explained. The hormic biologists rejoin, that bodily characteristics are never merely "given". They do not "happen to be". Instead, form is subservient to active purpose and shows a plasticity whereby function is maintained at an optimum despite alterations and distortions of form. The experimental work on this point is voluminous, but see, for example, Monakow and Mourgue (5) and Detwieler (9) who cite instances where disturbances in morphology result in a reorganization, as if reëstablishment of function were aimed at.

(6) By insisting on the importance of purpose, or goal seeking there is no denial of the importance of mechanistic explanation but only of its adequacy at the present time. The correlation of behavior with physico-chemical terms is necessary to bring the given phenomenon into relation with other simple phenomena. But to employ such concepts exclusively, neglecting the broader aspects of biology, does not at present give a comprehensive description of behavior. The future may possibly give some entity or chemical reaction which will completely explain (*i.e.*, correlate exactly and exhaustively) the definition of "horme" as given above. When such is the case, we can dispense with this vague concept in our general description and substitute the more exact. But until such an exact formulation is discovered, the vaguer concept, drawn from observation, must be retained (Tolman, 10). Consequently, hormic psychology presents no opposition to mechanism in point of method or proximal explanations. All the data are gladly adopted, but the unjustifiable extension of such data into inadequate theories is rejected.

The term "instinct" stresses ignorance, but if clearly defined and carefully used it implies something behind our ignorance, and shows the direction that future research must take. It is a cloak; what it covers is essential for understanding of behavior. What is under the cloak is the work of the future to find.

SUMMARY AND CONCLUSION

The concept of instinct implies that intervening between certain stimuli and certain responses there is, among other forces, one which is innate, propulsive, and concerned with purposes, ends, or goals. The hypostasizing of "instinct" as an explanatory concept has been widely attacked. But a brief consideration of scientific method shows that formally it is not inadequate.

From the viewpoint of content, proximal explanation is distinguished from general. Instinct is of no use in proximal explanation for any specific event; this should be in physico-chemical terms. But as a general term it implies a hormic biology which is a valid concept, not inconsistent with mechanistic data. By insisting on the importance of the goal, it unites separate disciplines of biology and forms a unified field of psychobiology where function is stressed. Such a "horme" can be used as a well-defined explanatory concept, with the merits of generality and absence of inconsistency. In the present state of our knowledge its adequacy counteracts its lack of simplicity. When the future produces a simpler, more exact concept of the same adequacy we can dispense with this vague term, but not until then.

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PERSONALITY DIFFERENCES BETWEEN NEGRO AND WHITE COLLEGE STUDENTS, NORTH AND SOUTH

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REVIEW OF LITERATURE

IN recent years there have been many studies made comparing Negroes and whites on intelligence scores. Few studies, however, have been made comparing the performances of Negroes and whites on the so-called personality tests. McFadden and Dashiell (13) in 1923 attacked the problem of personality of high school and college students, both Negroes and whites, using the Downey Will-Temperament test. They conclude, among other things, that "In 'strength of personality' as indicated by the total scores of each individual, the whites surpass the Negroes, only 15.4 per cent of the Negroes exceeding the mean of the whites." The whites are clearly superior in the number of "controlled" and "deliberate" persons tested, while the Negro group furnished a greater number of persons who combined "quickness" and "mobility" with "aggressiveness".

Sunne (14) in 1925 administered the Pressey X-O tests, Carnegie Test IV (Ream adaptation of Downey Will-Temperament Scale), Woodworth-Matthews' Questionnaire to 232 white and 282 Negro children in the seventh and eighth grades. The results obtained in some ways confirm McFadden and Dashiell's results, and in other ways not. From the results the writer felt justified in saying, "The differences between the racial groups examined cannot be summed up in such generalizations as more or less emotional, energetic, or unstable, or even by total scores on the tests." Differences within a race often appear as large as differences between races.

Hurlock (10), not satisfied with personality differences being so small in the McFadden and Dashiell results, since such large differences occur between the races in intelligence, decided that the scoring technique of the Downey Will-Temperament was perhaps at fault. Using gross scores instead of decile scores estab-

lished by Downey, she administered the test to 110 white and 101 Negro boys of the seventh and eighth grade levels in the public schools of New York. Her results show that "white boys are speedier in decision and movement than Negro boys, and that they possess greater volitional perseversation. . . . Negro boys, on the other hand, possess greater self-confidence, finality of judgment, motor inhibition and power of coördination of impulses". While her results agree fairly close with McFadden and Dashiell's, the author says, "The conclusion which the writer feels justified in drawing from the data presented . . . is that differences in temperamental traits do exist between Negro and white children. The finer the method of scoring temperamental tests, the greater the differences appear to be."

Cooper (4), using Allport's Ascendance-Submission Reaction Study, administered to 56 Negro college students (28 men and 28 women), found means and medians similar to Allport's norms. The Negro was not found submissive. Beckham (2), of Howard University, by means of a questionnaire sent to 2,000 Negro college students supposed to represent a fair sample of the Negro population, sought an answer to such questions as, "Is the Negro happy?" "Do you believe the Negro is happy?" Answers from different socio-economic class levels represented in his sample indicated that the majority of unskilled laborers say they are happy. Not so with Negroes representing the lawyer and preacher classes; they claim the Negro is not happy. Perhaps racial competition is at work here. Negro lawyers and doctors have had to compete with whites in these professions. They see the Negro in a different light from that of the unskilled laborers. Perhaps study has made them relatively more pessimistic or realistic. Beckham suggests adding one other to the already long list of complexes, namely *Negro Complex*. "This complex is not necessarily identical with Inferiority Complex", he says. "It is an emotional sublimation that enables the Negro to escape dire reality. In this state he is able to simulate happiness." Certainly, the writers claim, happiness or the lack of it is related to personality make-up.

Daniel (5) recently studied, in all, three hundred Negro cases of delinquent boys, problem boys, and non-problem boys in the public schools of Richmond, Virginia. He found Matthews' Questionnaire test, which is supposed to measure emotional stability, to differentiate between problem and non-problem boys. Other tests measuring character were also given. Bogardus (3), Graham (8), Guilford (9), and Lehman and Witty (12) have all

written upon some aspect or other of the Negro's personality adjustment as it is related to his environment.

As time goes by and as techniques develop, more refined results are obtained. Katz and Allport's (11) study, while not dealing directly with personality or with the Negro, throws light upon *social attitudes* among college students toward their own and other races. They call attention to studies showing that the Turk and the Negro arouse the greatest antipathy from other race groups in American colleges; further, that social attitudes may be stereotyped until symbols, names, or even faces become stimuli to arouse habitual emotional reactions between races. Droba (7) found in Ohio State University the white members of a Sociology class less favorable to Negroes than the few Negro students in the class were to whites. This would seem to indicate a condition ready to elicit maladjusting personality reactions on the part of the Negroes. If these prejudices do exist, may they not function in a way as to elicit "defense" or "inferiority" reactions on the part of the races toward whom they are directed? In this way, the Negro and the Turk would be let in for greater antagonism.

Katz and Allport, commenting upon the fact that Reinhardt's study shows West Virginia students agree with Bogardus' Pacific Coast subjects "in showing a greater prejudice against the Turk than against the Negro", have the following to say of sectional student prejudices: "On the whole a greater aversion was shown toward the Negro by northern than southern students, a finding difficult to interpret since it may be due either to actual facts of attitude or to the differentiating tendencies in geographical sections to express public as contrasted with private attitudes." The question might be raised if these degrees of aversion do exist, what influence may they have upon development of personality of the Negro in different sections? Do northern Negroes manifest a greater submissiveness than do southern Negroes? Again, do northern Negroes show a greater tendency toward introversion or toward extroversion? It is possible, of course, for a person to show more tolerance for a "class name" than for individuals who actually bear that name. It stands to reason, that the converse is true. Certainly, prejudices, if overtly expressed, must influence the personality development of individuals within the race having the prejudices as well as within the race toward whom the prejudices exist.

Abel (1) administered Washburn's questionnaire dealing with one's introspective account of emotions along with Woodworth's "Personal Data" sheet (abridged form) to Czech and Slovak

students, to French students, and to American students, in different universities of their respective countries to see if the nationalities represented differed in certain beliefs and preferences. While the Polish student tends to be more intellectual in his propensities, he also tends to be more neurotic than the French and Czech students. The Americans preferred athletic to intellectual pastime; they also tended to be less neurotic. The author suggests the differences found to be due to "environment", to "tradition", and to "inheritance".

About the cleanest cut piece of work in personality studies that the writers have seen, is that of Darrow and Heath (6), who recently approached the study of personality from two points of view, namely physiological and psychological. Using the galvanic skin reflex, blood pressure, respiration, etc., as physiological variables, the reactions of which were elicited by various stimuli, and comparing these reactions with the subjects' scores on Thurstone's Neurotic Inventory, and on Gilliland and Morgan's Northwestern Introversion-Extroversion Test, they sought an answer to the question as to whether physiological changes occurring in human subjects, following certain kinds of stimulation, may or may not be diagnostic of psychological personality reactions. In other words, will the *latent period* of deflection or the degree of *magnitude of deflection* of the galvanometer needle following certain kinds of stimulation reveal the same thing as test items on a neurotic inventory, or *vice versa?* From the above-mentioned measures, along with others, the writers conclude: "From the study of twenty-one measures of physiological reaction in relation (1) to neurotic inventory, (2) to the Northwestern University Introversion-Extroversion Test, (3) to the histograms showing the distribution of answers to the respective questions of the neurotic inventory, and (4) to composite scores obtained by compiling answers to groups of similar items from the neurotic inventory, we must conclude that in no instance have we demonstrated that temperamental type, as defined by questions used, is a result of the physiological traits measured" (p. 240). They did find, however, in some specific cases certain concomitant variations between psychological and physiological reactions. But the authors say of the related variables, "Those that correlate may be causally related, or both may be influenced by some as yet unknown factors" (p. 241). Studies like the one just mentioned make one less sure about measuring specific "traits of personality". This leads us to our own study.

PROBLEM

Whether there are inherently any fundamental differences between Negroes and whites on the temperamental or personality side is not known. The student of personality would be unwilling to say that there is a native difference; just as the mental testers after many years of study do not yet say that differences between Negroes and whites in intelligence are inherited. Darrow and Heath's study makes us even more wary of assuming a trait as a "psychological entity" with its accompanying physiological processes. It seems that most personality tests of the inventory type are merely an arrangement of a set of items or questions which center about certain socially observed and labeled reaction patterns or tendencies. These items are then standardized in a test form and presented to subjects in order that they may rate themselves. Their scores are then evaluated in terms of the norms. With this in mind the investigators have taken the Bernreuter Personality Inventory and applied it to Negroes and whites to see if it will differentiate the races on the so-called traits which this test is supposed to measure. If it does measure these traits, to what degree does the northern Negro differ from the southern Negro? If they differ sectionally, do they also differ racially from whites in these two sections?

Theoretically, at least, one may assume that Negroes who have grown up in white society where admitted prejudices and antagonisms exist (unless, of course, no insight be had into their adjustment relationship), that these prejudices and antagonisms toward the Negro, on the part of the whites, will be reacted to by the Negroes in such a way as to influence their personality development. If this be true, then, one would expect to find the Negro's reaction to certain items about personality adjustment to show the influence of racial antipathies; and, perhaps in addition to this, the influence of his socio-economic status. If these hypotheses be true, will the above-mentioned influences be revealed by such a questionnaire as Bernreuter's Personality Inventory when it is used upon college students?

SUBJECTS AND TESTS

Two tests were administered, the Otis Self-Administering Mental Ability test (30 min.) and the Bernreuter Personality Inventory, at the same sitting to college students in four universities; two in Ohio and two in Alabama. Two hundred and four whites, male and female, were taken from Ohio University and

the University of Alabama. The one hundred and seventy-seven Negro college students came from Wilberforce University * in Ohio and Tuskegee Institute in Alabama. Since Bernreuter's norms are for males and females separately, the numbers of each sex from the different schools are as follows: There are 39 white males from Ohio University and 54 white males from Alabama University; there are 64 white females from Ohio and 47 white females from Alabama; there are 79 Negro males from Wilberforce University (Negro University in Ohio) and 20 Negro males from Tuskegee Institute (Negro school in Alabama); there are 46 Negro females from Wilberforce University and 32 Negro females from Tuskegee Institute.

These subjects represent all four college classes with Freshman and Sophomore classes furnishing relatively the greater proportion. The average age of the whites is about twenty years; that of the Negroes a little higher. The Otis test was given only as a check. As far as we are able to ascertain, the cases are representative samples of the schools and sections. The instructions in the Manuals were followed in administering these tests. The Bernreuter Personality Inventory [1] was used to measure the personality "traits" of Negroes and whites. Since little has been published on this test, perhaps a few quotations from the manual will orientate the reader as to the nature and purpose of the test. We quote as follows:

"The *Personality Inventory* represents a new departure in the measurement of personality in that it measures several different aspects at one time. The immediate effect of this is a very considerable saving both in cost and in time required for administration. The fact that the nature of the traits being measured is not readily detectable and that the scales possess high reliability which permits their being used to compare one individual with another, are further distinct advantages.

"Four scales have been prepared and are now available. . . .

"B1-N. A measure of neurotic tendency. Persons scoring high on this scale tend to be emotionally unstable. . . . Those scoring low tend to be emotionally balanced.

"B2-S. A measure of self-sufficiency. Persons scoring high on this scale prefer to be alone, rarely ask for sympathy or encour-

* The writers wish to thank the Administration of Wilberforce University for coöperation in allowing us to use their students; likewise they wish to thank the Administration of Tuskegee Institute in Alabama for use of students there.

[1] Cf. Manual for Personality Inventory, Published by the Stanford University Press; also the Manual for the Otis Test, World Book Co.

agement, and tend to ignore advice from others. Those scoring low dislike solitude and often seek advice and encouragement.

"B3-I. A measure of introversion-extroversion. Persons scoring high on this scale tend to be introverted; that is, imaginative and tend to live within themselves. . . . Those scoring low tend to be extroverted; that is, they seldom worry, seldom suffer emotional upsets, and rarely substitute day dreaming for action.

"B4-D. A measure of dominance-submission (items in this test are based upon Allport's Ascendance-Submission Reaction study). Persons scoring high on this scale tend to dominate others in a face-to-face situation. Those scoring low tend to be submissive."

The Inventory is easy to administer, but the difficulty is in scoring. In fact, the most questionable thing about this *Inventory* in the minds of the writers, since using it, is the system of differential weighting of the same response. Willoughby (16) has called attention to this fact in a recently published paper concerning the Thurstone Neurotic Inventory. A definite evaluation of this test awaits further published facts about it. In a sense, one of the purposes of this investigation is to try out the test. Symonds (15) states that this Inventory is an integration of various questionnaires. As one means of validating the Inventory, Bernreuter correlated the scores of the four "traits" against (1) the Thurstone Neurotic Inventory, (2) the Bernreuter Self-Sufficiency Test, (3) the Laird C-2 Introversion Test, and (4) the Allport Ascendance-Submission Reaction Study. The coefficient of correlations were all high.

RESULTS AND DISCUSSION OF PERSONALITY VARIABLES [2]

The ages of the college students used in this study are in line with the average run of college samples picked at random. The means and standard deviations of the Otis scores are shown in Table 1 below.

The northern whites show a higher mean on the Otis test than

TABLE 1

INTELLIGENCE TEST (OTIS) SCORES, NEGROES AND WHITES

	North		South	
	Mean	S. D.	Mean	S. D.
Negroes	45.75	7.84	37.10	7.72
White	52.16	6.88	50.70	8.70

[2] The writers wish to express their appreciation to Mr. L. L. Henninger of the Psychology Department in Ohio University for aid in the statistical work of this research.

do the southern whites by a narrow margin of 1.46 points. There is a slightly greater tendency for the scores to scatter among southern whites. Both white groups show higher mean scores than do the Negroes. The northern Negroes have a mean score in intelligence of 6.41 points lower than northern whites; and 4.95 points lower than southern whites. The southern Negro group is much lower than any of the other groups. The mean of this group is 15.06 points lower than northern whites, 13.60 points lower than southern whites, and 8.65 points lower than northern Negroes. Between the means of all groups, except the mean between northern and southern whites, the differences are significantly reliable. The standard deviations of both groups of Negroes are about the same. The intelligence levels of these groups check fairly well with many other studies comparing Negroes and whites, north and south.

Knowledge of sex differences in the so-called intelligence tests results is becoming more refined as additional light is thrown upon the different aspects of performances of males and females on tests for that nature. Differences are still revealed when certain sensory-motor and associative learning test materials are presented. Perhaps the female is more easily motivated, or perchance, as has been suggested, the neural pathways involved are shorter. Motivation certainly involves its temperamental or emotional aspects. We have examined our data for sex differences on the "traits" incorporated in Bernreuter's Inventory, as Tables 2 and 3 will show.

TABLE 2

SEX DIFFERENCES IN PERSONALITY SCORES OF NEGROES, NORTH AND SOUTH

	Men N = 99			Women N = 78					
	M	σ	σ_M	M	σ	σ_M	diff. (means)	$\sigma_{diff.}$	$\frac{\text{diff.}}{\sigma_{diff.}}$
B1-N	—61.30	59.70	6.00	—21.30	75.90	8.59	40.00	10.48	3.82
B2-S	36.46	41.66	4.18	11.03	44.94	5.07	25.43	6.57	3.87
B3-I	—30.20	38.80	3.89	— 9.00	41.80	4.73	21.20	6.12	3.46
B4-D	66.00	50.40	5.06	34.20	50.40	5.70	31.80	7.62	4.17

TABLE 3

SEX DIFFERENCES IN PERSONALITY SCORES OF WHITES, NORTH AND SOUTH

	Men N = 93			Women N = 111					
	M	σ	σ_M	M	σ	σ_M	diff. (means)	$\sigma_{diff.}$	$\frac{\text{diff.}}{\sigma_{diff.}}$
B1-N	—43.74	71.65	7.43	—10.86	77.30	7.34	32.88	10.44	3.15
B2-S	16.02	52.61	5.46	— 0.27	60.14	5.71	15.75	7.90	1.99
B3-I	— 8.10	49.58	5.14	—12.27	46.44	4.43	4.17	6.79	.61
B4-D	44.36	54.77	5.68	18.78	60.95	5.78	25.58	8.10	3.16

Between all Negro males, north and south, and Negro females of both sections there are statistically reliable sex differences on all four traits, as the last column in Table 2 will show. Since the mean scores of the Negro males are higher on B2-S and B4-D than the Negro females, if Bernreuter's interpretations of the norms be correct, then the Negro males tend more toward self-sufficiency and dominance than do the Negro females. On the other hand, the Negro females tend more toward neuroticism and introversion than do the Negro males. However, the Negro females in all traits except one (B4-D) show a greater amount of variability.

Statistically reliable sex differences between all white males, north and south, and all white females from the two sections are found on only two traits, B1-N and B4-D (Table 3). The white females tend to be more neurotic, while the white males tend to be more dominant. There is a mild tendency for the white males to manifest, to a greater degree, self-sufficiency than the white females. In all cases except one (B3-I) the white women show a slightly greater variability than do white men.

In general, we may say that sex differences do exist between the males and females, both Negroes and whites, and that the males, on the whole, manifest a greater tendency toward dominance and self-sufficiency, while the females tend more toward neuroticism and introversion. On the last named trait, however, the white females show scarcely any differences from the white males.

While sex differences are of interest in these groups, yet our chief interest is racial. Most studies have shown the Negro to be inferior to whites on intelligence test scores, and some few studies, it will be noted from the literature summarized in the first pages of this paper, reveal differences in personality reaction tendencies. For the purpose of seeing if there are personality differences between races as measured by the Bernreuter Inventory, we compared all Negroes, both sections, with all whites, both sections. Further we have compared our Negroes with Bernreuter's norms. This last named group of whites comes chiefly from the Pacific Coast region.

Tables 4 and 5 will show these comparisons. Table 4 shows statistically reliable racial differences between the means of our Negroes and whites on only one trait, namely, introversion. Our white males tend to be more introverted than the Negro males. However, there is a near approach to the conventional reliability

TABLE 4

RACIAL DIFFERENCES IN PERSONALITY SCORES BETWEEN OUR TWO GROUPS, NEGROES AND WHITES

		Negroes N = 99 men, 78 women				Whites—Patrick & Sims N = 93 men, 111 women				Significance of differences between Means			Significance of differences between Sigmas		
		M	σ_M	σ	σ_σ	M	σ_M	σ	σ_σ	diff. (means)	$\sigma_{diff.}$	diff. / $\sigma_{diff.}$	diff. (sigmas)	$\sigma_{diff.}$	diff. / $\sigma_{diff.}$
B1-N	Men	—61.30	6.00	59.70	4.24	—43.74	7.43	71.65	5.25	17.56	9.55	1.84	11.95	6.75	1.77
	Women	—21.30	8.59	75.90	6.07	—10.86	7.34	77.30	5.19	10.44	11.30	.92	1.40	7.99	.18
B2-S	Men	36.46	4.18	41.60	2.95	16.02	5.46	52.61	3.86	20.44	6.88	2.97	11.01	4.86	2.27
	Women	11.00	5.07	44.80	3.85	— 0.27	5.71	60.14	4.04	11.27	7.64	1.48	15.34	5.58	2.27
B3-I	Men	—30.20	3.89	38.80	2.75	— 8.10	5.14	49.58	3.64	22.10	6.45	3.43	10.78	4.56	2.36
	Women	— 9.00	4.73	41.80	3.34	—12.27	4.43	46.44	3.12	3.27	6.48	.50	4.64	4.57	1.02
B4-D	Men	66.00	5.06	50.40	3.85	44.36	5.68	54.77	4.02	21.64	7.61	2.84	4.37	5.57	.78
	Women	34.20	5.70	50.40	4.03	18.78	5.78	60.95	4.09	15.42	8.12	1.90	10.55	5.74	1.84

standard between Negro men and white men (Table 4) on two other traits, self-sufficiency and dominance.

On both of these traits the Negro males show a more pronounced tendency. The fact that the Negro tends to show dominance to a greater degree than whites is in line with other published studies. While the differences are not reliable, yet it is interesting to note that the white women score higher on the neurotic scale than do the Negro women, while the Negro women show higher mean scores on the dominance scale than do the former. Just why our white males tend to be more introverted than Negro males is difficult to explain. The white males from Ohio University (a small group) came chiefly from students taking Educational Psychology, preparing to teach.* Perhaps the profession attracts that kind of personality. If this difference in introversion is a true one, the interpretation would mean that white males tend to be less able to face reality than Negro males. But, reality for the whites, may be relatively different than for Negroes. In all cases, both male and female, the whites show more variability in performance on this Inventory. This would seem to indicate that the Negro college student belongs to a more homogeneous group in so far as personality goes.

In Table 5 will be found a comparison between our Negroes and Bernreuter's whites on the personality traits.

Between these two groups on only one trait, dominance, do we find a statistically reliable difference. Again, the Negro males show more of the trait of dominance than do Bernreuter's white male subjects. Table 5 further shows a strong tendency for Negro males to be more self-sufficient than white males. There are about 97 chances in 100 that the difference between the Negro females and white females in neuroticism is a true one; while the chances are about 99 in 100 that the difference is a true one between the two groups of women in introversion. The Negro women tend more towards neuroticism and also more towards introversion. Here again on all traits Bernreuter's whites show a greater degree of variability than do our Negro group.

In Table 6 we have compared our whites with those of Bernreuter. This comparison was made to see if sectional differences between the two groups are found. In two cases we find statistically reliable differences between the mean scores of these groups. The women of our white group score higher on the neurotic scale than do the women in Bernreuter's group; also, our white male

* The writers are not positive about the professional interest of some of the other groups.

TABLE 5

RACIAL DIFFERENCES IN PERSONALITY SCORES BETWEEN OUR NEGROES AND BERNREUTER'S WHITES

		Negroes				Whites—Bernreuter				Significance of differences between Means			Significance of differences between Sigmas		
		M	σ_M	σ	σ_σ	M	σ_M	σ	σ_σ	diff. (means)	$\sigma_{diff.}$	$\frac{diff.}{\sigma_{diff.}}$	diff. (sigmas)	$\sigma_{diff.}$	$\frac{diff.}{\sigma_{diff.}}$
B1-N	Men	—61.30	6.00	59.70	4.24	—52.90	4.10	85.20	2.90	8.40	7.26	1.16	25.50	5.14	4.96
	Women	—21.30	8.59	75.90	6.07	—39.60	4.40	78.90	3.10	18.30	9.65	1.90	3.00	6.82	.44
B2-S	Men	36.46	4.18	41.60	2.95	24.90	2.60	54.00	1.80	11.56	4.92	2.35	12.40	3.46	3.58
	Women	11.00	5.07	44.80	3.85	6.90	3.10	55.70	2.20	4.10	5.94	.69	10.90	4.43	2.46
B3-I	Men	—30.20	3.80	38.80	2.75	—27.40	2.50	51.60	1.80	2.80	4.62	.61	12.80	3.29	3.89
	Women	— 9.00	4.73	41.80	3.38	—20.80	2.60	46.50	1.80	11.80	5.40	2.19	4.70	3.79	1.24
B4-D	Men	66.00	5.06	50.40	3.85	46.30	3.30	67.40	2.30	19.70	6.04	3.26	17.00	4.48	3.79
	Women	34.20	5.70	50.40	4.03	33.10	3.60	63.50	2.50	1.10	6.74	.16	13.10	4.74	2.76

subjects score higher on the introversion-extroversion scale than do his male subjects.

Thus we find reliable sectional differences between white college students on two traits, whereas the Negroes showed reliable differences between each of the white groups on only one trait. However, the reliable differences between the Negroes and the two white groups, in both cases, were between the male subjects, while between the two white groups we have one reliable difference between males (B3-I) and one between the females (B1-N). Further, there is a rather definite tendency (about 98 chances out of 100 that it is a true difference) for Bernreuter's women subjects to be more dominant than ours. On no trait do we find statistically reliable differences in variability, although there is a definite tendency in that direction between the males on both neurotic and dominant traits; his males showing greater variability.

In order to see if there are any sectional and racial differences between our four college groups of both sexes, Table 7 was prepared. In the last column of Table 7 on only one trait do we find a statistically reliable difference, namely, that of introversion. The white northern males tend more toward introversion than do the white southern males. There is a rather pronounced tendency, also, for the northern Negro females to be more neurotic than the southern Negro females.

Again, there is a tendency for white northern women to be more self-sufficient than white southern women. Perhaps, after all, the "clinging-vine type" of southern lady is not entirely a myth. The northern Negro females tend to be more introverted than southern Negro females; but the southern Negro males tend to be more dominant than northern Negro males. On the other hand, southern Negro females tend to be more dominant than northern Negro females; while the white northern females tend more toward dominance than do southern white females. The last statement seems in line with the one about northern white females being more self-sufficient than their southern sisters. In all cases mentioned, exclusive of the one showing statistically reliable differences between northern white men and southern white men on introversion, there is a high probability that these differences are not due to chance.

PERSONALITY AND INTELLIGENCE

The literature shows that coefficients of correlations between so-called personality traits and intelligence to be rather negligible; yet in order to see if there is any relationship between the four

TABLE 6

A Comparison between Our Whites and Bernreuter's Whites

		Whites—Patrick & Sims N = 93 men, 111 women				Whites—Bernreuter * N = 427 men, 317 women				Means			Sigmas		
		M	σ_M	σ	σ_σ	M	σ_M	σ	σ_σ	diff. (means)	$\sigma_{diff.}$	diff. / $\sigma_{diff.}$	diff. (sigmas)	$\sigma_{diff.}$	diff. / $\sigma_{diff.}$
B1-N	Men	—43.74	7.43	71.65	5.25	—52.90	4.10	85.20	2.90	9.16	8.49	1.08	13.55	6.00	2.26
	Women	—10.86	7.34	77.30	5.19	—39.60	4.40	78.90	3.10	28.74	8.56	3.36	1.60	6.05	.26
B2-S	Men	16.02	5.46	52.61	3.86	24.90	2.60	54.00	1.80	8.88	6.05	1.47	1.39	4.26	.33
	Women	— 0.27	5.71	60.14	4.04	6.90	3.10	55.70	2.20	7.17	6.50	1.10	4.44	4.60	.97
B3-I	Men	— 8.10	5.14	49.58	3.64	—27.40	2.50	51.60	1.80	19.30	5.72	3.37	2.02	4.06	.50
	Women	—12.27	4.43	46.44	3.12	—20.80	2.60	46.50	1.80	8.53	5.14	1.66	.06	3.60	.02
B4-D	Men	44.36	5.68	54.77	4.02	46.30	3.30	67.40	2.30	1.94	6.57	.30	12.63	4.63	2.73
	Women	18.78	5.78	60.95	4.09	33.10	3.60	63.50	2.50	14.32	6.81	2.10	2.55	4.79	.53

* The last decimal in all comparisons involving Bernreuter's norms is not significant due to the fact that Bernreuter's constants were carried to only one decimal place. We are grateful to Bernreuter for furnishing us his data through a communication with him.

traits assumed by Bernreuter and intelligence in the present study, the writers worked out Pearson r's between the Otis raw scores and percentile rank on the four Bernreuter scales. The coefficients of correlation are so low that for the sake of space the writers are not including the table. However, there are a few low relationships which seem worthy of mentioning, although they are not significant. The trait of self-sufficiency showed the highest positive relationship with intelligence in the northern white (both sexes) group. The coefficient is .21±.07. The coefficient of correlation for all Negroes (Ohio and Alabama) between self-sufficiency and intelligence is .10±.05. The white southern group (both sexes) showed a correlation between the neurotic tendency and intelligence of .10±.07. There is a negative relationship of −.11±.07 between the neurotic tendency and intelligence in the northern whites (Ohio, both sexes).

The above-mentioned trends and relationships between intelligence and personality arouse in one a little speculative thinking. Certainly from an observational standpoint some students of personality associate intelligence with personality. In fact intelligence is considered as a variable or trait of personality by many writers. Yet there seems to be practically no relationship between the two when measured by the different tests. Certainly the tests used must tap for different things. Within a geographic area differences between intelligence of whites and Negroes have been found. Between geographic sections differences within races and between races have been found. Yet personality test results (in the study here reported as well as in other studies previously reported) do not show uniformly consistent racial or sectional differences. Is it possible that the items which go to make up intelligence tests depend, on the whole, upon training in an *abstract verbal* or *symbolic* way? In other words, is there a *school-cultural-pattern,* as the sociologist would say, which is measured, in varying degrees, depending somewhat upon the test used and somewhat upon schooling or upon the school influence which the subjects tested have had? When this school influence is measured, northern students of both races (each race relative to his own) show a superiority, on the whole, over southern students. The level of this *school-influence-pattern* depends chiefly upon training in school subjects, books, and literature directly, or to the influence radiating from these things indirectly. When we come to the measurement of personality, the items which elicit reactions from the subjects come not so much from books or bookish things but they deal chiefly with *common every-day*

TABLE 7

SECTIONAL AND RACIAL DIFFERENCES IN PERSONALITY SCORES, NEGROES AND WHITES

	North			South					
	M	σ	σ_M	M	σ	σ_M	diff. (means)	$\sigma_{diff.}$	diff. / $\sigma_{diff.}$
B1-N									
Negro Males	—61.96	61.29	6.90	—62.50	54.87	12.27	.54	14.08	.04
White Males	—46.78	69.12	11.07	—41.56	73.35	9.98	5.22	14.90	.35
Negro Females	—11.44	68.52	10.10	—38.50	61.38	10.85	27.16	14.82	1.83
White Females	—13.18	83.13	10.39	— 7.69	67.98	9.92	5.49	14.37	.38
B2-S									
Negro Males	36.76	42.06	4.73	35.26	39.92	9.16	1.50	10.31	.15
White Males	12.56	50.88	8.15	18.52	53.70	7.31	5.96	10.95	.54
Negro Females	8.14	51.06	7.53	13.76	37.56	6.64	5.62	10.04	.56
White Females	5.62	63.04	7.88	—11.28	53.86	7.86	16.90	11.13	1.52
B3-I									
Negro Males	—29.10	40.20	4.52	—35.00	32.32	7.23	5.90	8.53	.69
White Males	11.28	49.58	7.94	—21.38	45.14	6.14	32.66	10.04	3.25
Negro Females	— 2.60	40.56	5.98	—16.62	42.34	7.48	14.02	9.58	1.46
White Females	— 8.12	49.84	6.23	—14.62	42.70	6.23	6.50	8.81	.74
B4-D									
Negro Males	62.46	52.65	5.92	76.50	41.85	9.36	14.04	11.07	1.27
White Males	48.09	55.11	8.82	41.67	54.36	7.40	6.42	11.51	.56
Negro Females	26.72	51.00	7.52	45.00	48.03	8.49	18.28	11.34	1.61
White Females	21.09	67.32	8.42	7.97	50.73	7.40	13.12	11.21	1.17

adjustment problems; problems dealing with concrete face-to-face situations with little abstract symbolic relationships being involved; problems met on the job, in-the-home, on the street, in a word, situations developing the *temperamental and emotional side of human nature.* When tests are made which tap for this kind of meaningful reactions, the results do not show sectional and racial differences to stand out as clearly as they do when intelligence tests are given. May it not be said, then, that there is another cultural pattern which is more universal than schooling, namely a *personality-adjustment-cultural-pattern,* which involves likes and dislikes, fears and hates, and which is chiefly concrete; in these things Negroes and whites, perhaps all races, do not differ so much? Again, judging from the reactions of the Negro college students to this *pencil and paper* test in their own Negro college situation, they answer these items in a similar way to that of white college students. Whether their reactions in a face-to-face situation would bear out their opinion of how they do is another matter. Or whether, if they were in a white college, the answers to these items would place them in the same position on the personality scale, we do not know.

In summarizing, our contention then in our two hypotheses, which seems to be borne out, partially at least, by facts, is that the Negro functions on a lower level than the whites when the nature of the situation * is chiefly abstract, that is, it deals with symbolic representations (test items) of a school influence nature, but that when the situation confronted, in the form of test items, although symbolically represented, deals chiefly with concrete social adjustments of every-day life, then the superiority of the whites over the Negro is not so consistently shown. On the other hand, great differences in personality traits between the Negro and white might be revealed if the Negros and whites were brought into a more intimate face-to-face relationship. This last statement may be inferred from the fact that there are admitted prejudices between the two races. Without entering into the controversy between the "g" factor and "specific" ability protagonists the writers, in the above discussion, are not assuming that intelligence is not involved in personality adjustment. On the contrary, they feel that it is; what they are attempting to do is to call attention to the nature of the items in the two kinds of measuring rods and to emphasize that these items represent different situations which involve perhaps different degrees of discriminatory reac-

* Whether his lower performance to abstract things is due to inheritance or to lack of specific training is another matter.

tions on the part of the subjects examined. If the items in a personality inventory involved less obvious and less common meaningful reactions and more subtle language discriminatory features, and the person tested had fewer established modes of adjustment in this field, there would probably be a higher correlation between the results obtained between intelligence and personality tests.

The above discussion makes no pretense of being complete or final. It is offered only to stimulate further analysis of the relationship between personality and intelligence; also, to reëmphasize the fact that the reaction of an organism is conditioned by the nature of the situation to which he is reacting.

SUMMARY AND CONCLUSIONS

1. The literature shows that some investigators find race differences on some personality traits while other investigators find differences on other traits. None of the studies show as definite consistent differences in personality between Negroes and whites as have been found in intelligence. Many studies show that Negroes tend more toward dominance and aggressiveness than whites but that they are lacking in persistent deliberation. Several studies have shown that racial prejudices do exist between Negroes and whites. It might be inferred that these prejudices, if expressed in conduct toward the Negro, would tend to create a social environment which would influence the development of his personality. Our present study, however, does not reveal any pronounced influence of those prejudices. Moreover, the attitudes of prejudices found on the part of white college students toward the Negro may have been built up through reading and verbal experience without the students' having had actual contact with Negroes.

2. The present study reports racial and sectional differences in intelligence as have been found by other investigators. Northern whites tend to score higher on the Otis than do southern whites; likewise northern Negroes tend to score higher on this test than do southern Negroes. Further, northern whites score higher than northern Negroes; also, southern whites score higher than southern Negroes. In all cases the differences between the means are significantly reliable, except between the means of northern and southern whites.

3. Reliable sex differences are found between the sexes of both races. On all four traits the male and female Negroes differ. The Negro males tend to be more dominant and more self-sufficient than do the females, while the females tend more toward

neuroticism and introversion than do the males. Reliable sex differences are found on only two traits in the whites. The white males tend more toward dominance than do the white females, while the females tend more toward neuroticism than the males.

4. Statistically reliable race differences between the Negroes and our white group are found on only one trait, namely introversion. The white males tend more towards introversion than do the Negro males. There is a strong tendency, however, for the Negro males to be more self-sufficient and more dominant than the white males. There is a tendency, also, for white women to score higher on the neurotic scale than the Negro women; yet the latter tend more toward dominance than the former. The whites show more variability than do the Negroes. This would lead one to infer that the Negroes are a more homogeneous group from a personality point of view.

5. Between the Negroes and Bernreuter's whites there is only one trait which meets the conventional criterion of reliability in difference, that of dominance. The Negro males are more dominant than the white males, according to the Bernreuter Inventory. There is also a strong indication that the Negro males are more self-sufficient. The Negro women tend more toward neuroticism and toward introversion than the white women in these two groups. Again, Bernreuter's whites tend to be more variable than the Negroes.

6. Upon comparing our white group with that of Bernreuter we find statistically reliable differences on two traits, neuroticism and introversion. The white women in our group tend more toward neuroticism than do those in Bernreuter's group; our white males tend more toward introversion than do his males. Both differences are statistically reliable. This indicates (if our samples are representative) reliable sectional differences within the white race. The women in Bernreuter's group tend more toward dominance than do the women in our group. There is not a reliable difference in variability, however, between these two groups of the same race.

7. Table 7 shows racial and sectional differences between the groups from the four schools. The last column in Table 7 shows only one reliable difference between all subjects on the four traits. The northern white males show introversion to a greater degree than do southern white males. This difference meets the conventional standard of reliability. There are several differences between these groups which approach the conventional statistical significance, as follows: Northern Negro women tend to be more

neurotic than do southern Negro women; northern white women tend more toward self-sufficiency and toward dominance than do southern white women; northern Negro women tend more toward introversion than do southern Negro women; southern Negro males tend to be more dominant than do northern Negro males. In all cases there is a greater than chance probability of a true difference.

8. These results can be taken only tentatively for further confirmation or refutation. But on the whole they seem to indicate that the Negro in a Negro college situation answers the items in this personality Inventory in a similar fashion to the white student in his own college situation. Whether the two types of tests, intelligence and personality, are tapping for meaningful reactions in two aspects of experience, the intelligence test in the realm of the more abstract-school-influence which requires a superior or at least a different type of discrimination, and the personality in the realm of the concrete-social-likes-and-dislikes which requires less fine discriminations, remains yet to be proven. Certainly, results in the field of personality thus far do not show that whites maintain consistently the superiority over the Negroes that they do in the field of intelligence.

9. Bernreuter's inventory does differentiate to a certain degree. Whether the differences prove valid or not depends upon future research.

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PERSONALITY TRAITS ASSOCIATED WITH VARIATIONS IN HAPPINESS

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INTRODUCTION

IN a recent autobiographical sketch by a past president of the American Psychological Association, the following passage appears:

"I am desirous of participating in the solution of one more problem in applied psychology. That is the problem of protracted human happiness. Whether or not it fits in with one's philosophy of life, the fact is incontestable that happiness is an important if not the most important aim of human endeavor. Notwithstanding this fact, it has received no commensurate scientific attention. The theory of the happy life remains at about the level where Greek philosophers left it. There has been an immense amount of ameliorative activity and human welfare work, but it is practically all a treatment of symptoms without fundamental analysis. We are trying to correct a number of the supposed major conditions of unhappiness. Personnel studies try to avoid putting round pegs in square holes. Studies of family life, like that of Hamilton's, try to develop adaptive behavior in the smallest social group. Medicine and hygiene try to cure and prevent ailments of body and mind. But these symptomatic and ameliorative activities touch only small sections of human unhappiness.

"Scientific information as to the fundamental positive conditions of protracted happiness are conspicuous for their absence. We do not even know the laws by which pleasant situations become unpleasant and unpleasant situations become bearable or even pleasant. The positive conditions of happiness are left largely to accident, such as the satisfaction of instinctive wants with its tragic disillusionments and negative adaptations, the economic pressure to provide a market for manufactured products, the exigencies of the labor market, the desire to amass wealth, or the abolition of private wealth. There are numerous wise protests that protracted happiness is achieved by none of these things; but positive scientific data on the real conditions are as inconspicuous as scientific interest in the problem. If there were a real solicitude for intelligent adaptation, our science would be busy with very different tasks."(2)

As early as 1906, James Mackaye (5) tried to develop a specific "technology of happiness", but the same lamentable disregard of fundamental issues which keen observers have noted in American

life has also led to a neglect of the brilliant suggestions contained in this solitary genius's writings. If happiness be one of the major goals of living, if not the only consciously acceptable end of life itself (most people in practice behave as though they were hedonists or eudaemonists), surely an analysis of the conditions fostering or hindering its attainment is an intellectual obligation of the first order, since upon it rests the merit of all other human and social values.

Myerson (7) has advocated a program of "eupathics (the more gracious sister of eugenics)" by means of which the mood of mankind may be elevated. There are few minds, lay or academic, who would dispute Powys's (9) statement, "We cannot claim that Nature puts her seal of approval upon our culture until we can prove that it results in greater happiness for the individual who practices it." Yet despite an enormous body of literary (4), ethical (3), sociological, philosophical (6), and popular (8) discussions bearing upon the theme of happiness, there are apparently only two inductive studies of the problem which have employed the techniques of modern psychology.

Goodwin Watson (12) reports an investigation based upon the happiness self-estimates of 388 graduate students of education. These self-estimates were supplemented by extensive personal and biographical data of the social census type which were statistically analyzed for their bearing upon the happiness rating. Some interesting tentative conclusions were reached, particularly those concerning the apparent causes of happiness or unhappiness:

A. The essentials of happiness, for most people, are among the stable elements of life (friends, work, nature), not among the stimulants (alcohol, clubs, churches, dancing, cards, automobiles, arts).
B. Enjoyment of, and success in, work is a major factor in happiness.
C. Good health in childhood is the foundation for happiness.
D. Success in dealing with people is fundamental to happiness.
E. Love of nature goes with greater happiness.
F. The married are happier than the unmarried.
G. Failure in love is a major cause of unhappiness.
H. Those who look forward to living at age sixty alone, or with a sister, are predominantly unhappy.

Most of these results are highly plausible despite a crude quantitative formulation in terms of "more or less" since they harmonize beautifully with the wisdom of empiricism, but a number are disconcerting to "common sense" impressions, especially the finding that age, intelligence, knowledge of academic subject

matter, and popularity with the other sex seemed unrelated to happiness.

Sailer developed Watson's procedure on an even larger scale, securing 500 questionnaire returns from young men throughout the country. He found "evenness of temperament" to be most prominently associated with general happiness and the factor which best differentiated the happy from the unhappy. In addition, worry, ill-health, sex difficulties, were more conspicuous among the unhappy groups.

These two exploratory studies, while suggestive of some of the more potent *causes* of happiness, unfortunately give little hint of the personality traits found in conjunction with this mental state. The emphasis in these investigations is upon external or sociological factors conditioning happiness while the more subjective features are relatively neglected—an item of some significance since there is a strong probability that the pathways to happiness are varied and highly individual; many, in fact, have held that the sources of happiness are purely internal. Moreover, all the relations unearthed by Watson and Sailer were based upon self-estimates; no use was made of the device of rating by one's fellows. While it is true that happiness can at present be measured only in the form of ratings, this does not mean that it can be reported upon by none other than the individual affected. Assuredly happiness has its symptoms and manifestations in overt behavior which can be observed and appraised by acquaintances. Hence, in the present investigation, associates' estimates of felicity are used as well as self-ratings. Presumably a better index of happiness can be obtained by a proper combination of these two sources than through exclusive reliance upon a single one. If, against this united criterion, one sets the individual scores made on a selected group of newer personality measures, the possibility of discovering some unique relationships becomes enhanced.

PROCEDURE

The first task of the present study was to secure an adequate number of self-ratings from a fairly homogeneous group of 195 college sophomores pursuing a course in educational psychology. All the data presented below were collected during the month of May, 1932. Each person filled in privately the following blank with a graphic scale which is self-explanatory. These personal ratings were repeated a month later with part of the group in order to secure some check upon the consistency or reliability of their judgments.

SELF-RATING OF .....
(please insert your full name above)

DIRECTIONS: As part of a larger study in the psychology of the teaching process, will you please furnish the following estimate of yourself? The most accurate result will be obtained if you adopt a critical and dispassionate attitude toward your own life.

1. If you compare yourself with others of the same sex and age, how would you rate your own *general happiness*? Use this definition as a guide: "A relatively permanent state of well-being characterized by dominantly agreeable emotions ranging in value from mere contentment to positive felicity." Give due weight to both inner and outer factors or manifestations.

Place a cross (x) on the line below to show where you belong. This mark may be placed *between* as well as *at* the descriptive phrases. Think of your *average feeling or condition over several months*, and try to view yourself as objectively as possible.

most unhappy of all	the *great* majority of persons are happier than you are	a *slight* majority of other people are happier than you are	about average	somewhat happier than the general run of mankind	far happier than the great majority of human beings	happiest of all

2. Where do you think your *friends* would rate you? Make a circle (o) on the same line above to show where you think they would put you, whether at the same point at which you rate yourself, or higher, or lower.

ASSOCIATES' RATING OF .....
(Insert full name of person who gives you this)

DIRECTIONS TO THE RATER: This is part of a study which promises to have some significance for the psychology of the teaching process. You are asked to rate your friend confidentially and in good faith with respect to three highly important features. When you have done so seal the envelope and return it to the person you have rated who will in turn convey it to the appropriate instructor. Do not sign your name and remember that your judgments will not affect academically or otherwise the standing of either yourself or your friend. You are merely aiding in an interesting investigation.

I. HAPPINESS—If you compare him with others of the same sex and age, how would you rate your friend's *general happiness?* Use this definition as a guide: "A relatively permanent state of well-being characterized by dominantly agreeable emotions ranging in value from mere contentment to positive felicity." Give due weight to both inner and outer manifestations.

Place a cross (x) on the line below to show about where he belongs. This mark may be placed *between* as well as *at* the descriptive phrases Think of his *average feeling or condition over several months* and try to view him as objectively as possible.

most unhappy of all	the *great* majority of persons are happier than he is	a *slight* majority of other people are happier than he is	about average	somewhat happier than the general run of mankind	far happier than the great majority of human beings	happiest of all

II. EFFICIENCY—Place a cross on the line below which best represents your estimate of the subject's possession of the trait of efficiency. Be as fair and impartial as you can, and ask: Can he perform (and *does* he!) promptly, smoothly, and adequately a great many tasks, originating either with himself or with others? Remember that the cross may be placed at *any* point on the scale.

utterly incompetent to discharge any obligation	very lax and irresponsible	not as reliable as one would like	average capacity for managing affairs properly	handles matters with more than ordinary competence	responsible and reliable in executing assignments	extremely efficient in all duties

III. SYMPATHY—Place a cross on the line below which best represents your estimate of your friend's possession of the trait of *sympathy.* Ask yourself: To what extent does he participate in the emotional experiences of others, *i.e.,* how much *compassion* and *genuine kindliness* does he exhibit in his human relations?

wholly devoid of sympathy; brutal	callous and definitely unkind	indifferent	ordinary interest in others' problems	distress of others understood and appreciated	marked feeling of kinship with the "underprivileged"	borders on the saintly in word and act

* LOOK OVER EACH ITEM AGAIN AND SEE IF YOU HAVE BEEN AS *OBJECTIVE* AS POSSIBLE!

After the first self-rating, each subject was given four envelopes bearing his name and containing a longer rating sheet to be submitted to four friends whom he considered knew him well enough to judge him with respect to these traits. In addition to the happiness item this blank made provision on a similar basis for estimates of "efficiency" and "sympathy", both qualities which it was felt might have some bearing upon the degree of happiness possessed, and which at present can be measured only by this method (see the blank below). After collecting the sealed envelopes containing the ratings by his acquaintances (which usually required one week) the subject returned them to the experimenter. The novelty and intrinsic interest of the task insured excellent coöperation; precedent for the procedure may be found in the method employed by the Allports in standardizing their ascendance-submission scale.

It will be noted that each self and associate's rating contains a graphic scale with seven defined "classes". By considering each of the six intermediate spaces as the equivalent of an extra class, original scores ranging from 1 to 13 (always whole numbers) could be procured, thus providing as refined a measure as is feasible with this technique.

The next step consisted in building up the "joint happiness criterion", the fundamental numerical unit of this investigation. This was obtained by summing the two self-estimates with all four of the associates' estimates and dividing by six for the average rating. Such a method gives the friends' ratings as a whole twice as much weight as those of the person judged, but it appears more defensible than a procedure which would consider them equal. The resulting composite score was then correlated with the six phases of the Allport-Vernon scale of values blank, the three main divisions of the Bernreuter Personality Inventory, the DeCamp intelligence test, and Watson's public opinion booklet (a measure of extent of prejudice).

RESULTS

In Table I will be found the means and standard deviations of the happiness, efficiency, and sympathy scores as determined from the self- and associates' ratings:

TABLE I

MEANS AND STANDARD DEVIATIONS OF THE HAPPINESS, EFFICIENCY AND SYMPATHY RATINGS

	N	Mean	S.D.
Self-estimates of happiness	194	7.969	2.10
First self-estimates of happiness (sub-group)	92	7.924	2.26
Second self-estimates of happiness (one month later)	92	8.01	2.34
Self-estimates of others' views of one's happiness	182	9.017	1.66
Self-estimates of others' views of one's happiness (sub-group, initially)	88	8.227	1.66
Self-estimates of others' views of one's happiness (sub-group, one month later)	88	8.09	1.94
Associates' views of subject's happiness:			
First two acquaintances	179	8.145	1.48
Last two acquaintances	179	8.07	1.43
Joint Happiness Criterion	195	7.95	1.27
Associates' views of subject's efficiency:			
First two acquaintances	180	8.79	1.78
Last two acquaintances	180	8.96	1.68
Associates' views of subject's sympathy:			
First two acquaintances	181	8.58	1.37
Last two acquaintances	181	8.64	1.38

It is to be observed that the means for each item are uniformly greater than 7.00, the central point on the graphic scale. Seventy-five per cent of the group rated themselves as possessing happiness equaling or exceeding that of the average; 86 per cent placed their friends' estimates of their (the subjects') happiness at average or above; 96 per cent were said by their acquaintances to reach or exceed the mean for efficiency; and 94 per cent of the group were rated as having average or more than average sympathy. This indicates either that college undergraduates are happier, more efficient, and more sympathetic than the mass of mankind, or, as seems more likely, that there is a constant tendency to overestimate. If so, it is a fortunate, if curious fact that most people consider themselves (and are deemed by others) happier than the mass of mankind. But contrary to expectation, most subjects do not believe that others think them relatively much happier than they (the subjects) believe themselves to be, although a number of such cases occur.

Sex differences are a constant source of interest, even though the vast literature on this theme reveals no significant mental features unique to either male or female. Table II contains the information pertinent to this subject:

TABLE II

MEANS AND VARIABILITIES OF MEN AND WOMEN IN HAPPINESS RATINGS (JOINT HAPPINESS CRITERION)

	Mean	A.D.	N
Men	8.125	.86	84
Women	8.113	1.05	102

Clearly there is no reliable difference between the averages for the sexes, so that another old myth of parlor debaters and ardent feminists appears to be blasted. There is, however, an observable difference in dispersion, the women evidently being more likely to experience both the heights and depths of the happiness scale—a fact which Havelock Ellis may have had in mind when he spoke of woman's "greater emotionality".

Table III presents the more significant intercorrelations obtained from the rated traits. As anticipated, the highest r is between the two self-estimates of happiness taken one month apart. While this correlation is strictly a measure of stability or consistency rather than of reliability in the conventional sense, it can probably be interpreted as such without undue violence to the concept. The lowest r is between efficiency and sympathy, a plausible enough result since many persons in an effort to be efficient at all costs probably create an unsympathetic impression, this reducing the positive relationship normally found between most good qualities. Fair agreement among the opinions of friends is revealed by the r of one pair with another (.68), nearly as high as that of the self-estimates themselves, the value of which it theoretically could not exceed. At any rate, it is consoling to find that happiness can be more consistently rated than either efficiency or sympathy, a fact which is made more striking when one remembers that efficiency belongs to a class of traits that can be judged by more concrete and objective standards than most others. The most disconcerting fact is the low r of .34 between the self-rating and the mean of the associates' estimates of happiness. Evidently a wide disparity can exist between *attributed* and *experienced* felicity.

TABLE III

RAW INTERCORRELATIONS OF THE HAPPINESS, EFFICIENCY, AND SYMPATHY RATINGS OF APPROXIMATELY 200 COLLEGE SOPHOMORES

Correlated Variables	r	P.E.
First self-estimate and second self-estimate (one month later)	.70	.045
First self-estimate of others' views and second self-estimate of others' views (one month later)	.59	.06
First self-estimate of happiness and mean of associates' estimates thereof	.34	.04
Self-estimate of associates' estimates and actual average of associates ratings	.415	.04
Mean of first two associates' estimates of happiness and mean of last two	.68	.04
Joint happiness criterion and average of associates' ratings for efficiency	.383	.04
Mean of first two associates' estimates of efficiency and mean of last two associates' estimates	.57	.043
Mean of first two associates' estimates of sympathy and mean of last two associates' estimates	.53	.042
Joint happiness criterion and average of associates' ratings for sympathy	.303	.04
Efficiency average and sympathy average (estimates of acquaintances)	.232	.045

Table IV gives the correlations found between the joint happiness criterion and other standardized tests. These tests were administered with the hope that certain fairly definite features would be found to be associated with the possession of happiness, thereby making possible a multiple regression equation for predicting its degree in individuals from their known rank in other qualities.

TABLE IV

PRODUCT-MOMENT CORRELATIONS BETWEEN HAPPINESS AND OTHER PERSONALITY TRAITS

(N = 195)

Joint happiness criterion and:	
Neurotic tendency (Bernreuter)	= −.26 ± .04
Ascendance-submission "	= .19 ± .046
Self-sufficiency "	= .05 ± .05
Theoretical attitude (Allport-Vernon)	= .05 ± .05
Economic attitude " "	= .02 ± .05
Aesthetic attitude " "	= .00 ± .05
Social attitude " "	= .04 ± .05
Political attitude " "	= .06 ± .05
Religious attitude " "	= .15 ± .05
Intelligence (DeCamp)	= .13 ± .05
Teaching interest (Strong)	= −.02 ± .05
Prejudice (Watson)	= .09 ± .05

Most of these values are disappointingly low and only the ones for neuroticism and ascendance can be considered to lie beyond the conventional P.E. limits. This suggests that *emotional health* is the most important single factor associated with happiness, but it is far from supporting the view of the more enthusiastic mental hygienists that it is the *sole* producer of it. The dominant individual apparently has slightly greater chances of being happy than the submissive person, a finding which is hardly comforting to an advocate of traditional Christian ethics, but lends some credence to the analyst's claim that "happiness is an endogenous, *narcissistically* conditioned state that arises when the ego limits are widened by the setting up of a unity between the ego and the world by means of object cathexis, sublimation or unity within the self".(1) The absence of any relation with self-sufficiency indicates that the "rugged individualist" is no more likely to be happy than the "clinging vine", which suggests that the social factor is of much less importance than is commonly thought, and incidentally casts new light upon Weiss's view (13) that "neurologically the word happiness indicates the extent to which the innate and acquired components of sensori-motor function approach an optimum relationship between the antagonistic processes of individualization and socialization".

It is especially noteworthy that with the exception of the religious attitude none of Spranger's six value-types (as diagnosed by the Allport-Vernon profile) seems to be more conducive to happiness than any other. Evidently the nature of one's ideal is an irrelevant matter: the good, the beautiful, and the true are on equal footing as far as their capacity to foster bliss is concerned. To the writer's knowledge, this is the first experimental evidence against the Aristotelian ethic which held that "pure speculation" is the *summum bonum!* The term "religious" in Spranger's system may perhaps be rendered better by "philosophical", in which case it appears that there is a *faint* probability that the possession of a fixed *Weltanschauung* favors happiness, but there is nothing to suggest that adherence to orthodox beliefs is a force making for greater felicity. As Watson has earlier discovered, there is no demonstrable connection between intelligence and happiness, although it is not inverse as many defeatist cynics have preached. Intensity of prejudice on debatable social issues evidently does not aid or hamper the attainment of happiness. It is even more curious, however, that neither high nor low interest in the career for which one is preparing (these subjects were all prospective teachers) appears to be related to one's happiness as here determined—a fact which may well evoke critical inspection of some of the unchallenged principles of contemporary vocational counseling and the implicit utilitarian philosophy upon which they are largely founded.

CONCLUSIONS

With the possible exception of the finding that unhappy people are mildly inclined to be neurotic, most of the definite contributions of this study are negative in character. The low correlation between happiness self-estimates and the ratings of others is a fundamental limitation, which in conjunction with the inevitable factor of attenuation, is probably responsible for most of the negligible r's reported. Furthermore, a college population may be a poor group upon which to base generalized conclusions concerning humanity; *e.g.,* most of these subjects considered themselves happier than the average, which may be literally true in view of the relatively sheltered existence led by the American undergraduate. We are not justified in saying that the ordinary citizen believes himself happier than the ordinary person, although this is a legitimate hypothesis to investigate with a lay group. Similarly, the absence of any relation between estimated happiness and vocational interest may hold only for adolescents not yet

economically self-sustaining, but the weight of authority makes this seem improbable with mature professionally-engaged individuals. Perhaps, too, instead of using a "joint happiness criterion", all the measured variables should have been independently compared with the self-estimates taken alone and then with the acquaintances' ratings as such; in this way, consistencies or discrepancies would have been made doubly certain. The obviously tentative nature of the statements here made cannot be sufficiently emphasized, and every conclusion here reached needs to be subjected to the test of repetition with both younger and older groups of equal magnitude.

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THE STUDENT AND LAYMAN DEFINITION OF PSYCHOLOGY

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EVERYONE uses the word *psychology* constantly. Newspapers, magazines, books and even the radio daily inform the public that some action is best "psychologically", or that we have only to learn a few principles or rules in order to be a hundred or to earn a necessary million dollars. It is so sophisticated and fascinating that all pretend to be well versed and read on the subject. It seems that almost everyone knows what psychology is about, though no one knows what it is.

The recent strides of psychology have bred a type of individual who preys upon popular ignorance of the subject. In the 'Gold room' of some large hotel, or at a reputable church or club, he throws out his bait. Here he gives a free lecture on the evils of various complexes or a fundamental personality classification, the recognition of which is one's only chance of happiness. Once the listener is convinced of this, he is urged to subscribe to the private lectures, usually at twenty-five dollars a series. The imposing title of the 'quack' and the very name of the church, hotel, or club on his posters gives him a false prestige. The public naturally concludes that the lecture or clinic is being sponsored by the organization in whose halls it is held.

The serious student in the academic field has wondered what would be the results of this supposed "popularization" of psychology. In an attempt to discover the attitude towards psychology of a representative group, the writer made a study of two mid-western business concerns. The two groups of a total of one hundred and seventy-one individuals, apparently constitute a good cross-section of the average office force. Both groups contained department heads, assistants, typists, clerical workers, clerks, etc. Ninety-two individuals stated that they had taken one or more academic courses in psychology while seventy-nine stated they had never followed an academic course in the field.

In a preliminary study a hundred and seven individuals gave their own definition or general description of what they thought the subject of psychology consisted. While the general unpopularity of a form question was realized, it was felt that those indi-

viduals who did answer would give less prejudiced answers than if they were questioned in person. With no control over the answers, there resulted a great amount of duplication of responses. To offer some control on the final investigation, a committee of instructors in psychology summarized the answers into twenty-five phrases.

Using these twenty-five phrases the final form was prepared and presented with the following instructions:

"We want to know what *you* think is included in the field of psychology. Therefore, we have typed below a number of possibilities which we want you to read before you place a single mark. Then place a

1—before the phrase which you think is the best description;
2—before the phrase which you think is the second best description;
3—before the third best; and a
4—before the fourth best description.

If you think of a better description not listed below, write it in the space provided, and mark it as one of your four choices."

This form was then distributed to the one hundred and seventy-one individuals of the two concerns, of whom sixty-seven had given responses in the preliminary investigation. All could now select the four definitions they considered as the best.

Table I presents the *rank-order* of the definitions according to a point-weighting classification. A first choice was given a value of four points; a second choice a value of three points; a third choice a value of two points, and a fourth choice a value of one point. The rank-order of each item was then determined by the total points gained through first, second, third, and fourth choice votes. Where the total points for two items were tied, both items have the same rank-order and the adjustment is made by omitting the next rank-order. In the one case of three items having the same total points the median rank-order is quoted. Where an item received no first, second, third, or fourth choice votes, the rank-order is recorded as zero. Column one presents the entire group of one hundred and seventy-one individuals. Column two presents the sub-group seventy-nine individuals who had never taken a course in psychology. Column three presents the sub-group of ninety-two individuals who had taken one or more academic courses in psychology.

It is interesting to note that those individuals having no academic training in the field of psychology list the study of *the mind* as first choice while those having one or more courses list it as fourth. Again those individuals with academic training in

psychology list the study of *human behavior* first while those without academic training list it as third. The *reaction to mental stimuli* ranks as second for both groups.

TABLE I

CLASSIFICATION OF DEFINITIONS

(According to rank-order of preference)

Psychology Is the Study of	1 Entire Group	2 Without Training	3 With Training
1. the human brain and nervous system	5	7	3
2. the control of people	16	18	12
3. reactions to mental stimuli	2	2	2
4. the measurement of mental capacity	14	15	16*
5. the use of common sense	12	10	14
6. the interaction between people	11	17	10
7. the motives for activity	7	8*	7
8. human behavior	1	3	1
9. systematic thinking	13	8*	18*
10. mental diseases	0**	0**	0**
11. the science of habits	17*	16	15
12. the interaction between mind and body	4	4	5
13. the analysis of human traits	6	5	6
14. reactions to environment	8*	12*	8
15. the mind	3	1	4
16. personality	19	21*	13
17. how to train the mind to attain desires of owner	17*	14	20
18. heredity and environment	23*	0**	22*
19. the development of the mind	8*	6	11
20. the soul	22	19*	0**
21. vocational guidance	21	19*	22*
22. human nature	10	11	9
23. the sub-conscious mind	15	12*	18*
24. the difference between man and animals	23*	0**	22*
25. the bunk	20	21*	16*

* Column 1—Items 14 and 19 tied for eighth place; 17 and 11 for seventeenth; 24 and 18 for twenty-third.
Column 2—7 and 9 for eighth; 14 and 23 for twelfth; 20 and 21 for nineteenth; 16 and 25 for twenty-first.
Column 3—4 and 25 for sixteenth; 9 and 23 for eighteenth; 18, 21, and 24 for twenty-second.

** No 1st, 2nd, 3rd, or 4th choices.

The order in which the definitions fall for the high six, as measured by total points, is as follows for the two sub-groups:

Without academic psychological training	*With academic psychological training*
1. The mind.	1. Human behavior.
2. Reaction to mental stimuli.	2. Reaction to mental stimuli.
3. Human behavior.	3. Human brain and nervous system.
4. Interaction between mind and body.	4. The mind.
5. Analysis of human traits.	5. Interaction between mind and body.
6. The development of the mind.	6. Analysis of human traits.

Here we find that psychology has not lost its mystical study and development of *the mind* in the opinion of even those with academic training in psychology. Originally, psychology was a branch of philosophy, and the older dictionary definition—the science of the soul—was applicable. But a generation ago psychology attempted to sever its relations with philosophy and become a science in itself. It was found that the mind—or the soul—could not be defined or located in the earlier sense. In its place, whether through the study of consciousness or behavior, psychology has taken for its field all integrated activities of the whole organism from birth to death. It does not seem out of place, therefore, to suggest that more time in the future should be spent by the professional psychologist in thought and education as to what psychology is. Or, perhaps, a new name should be sought that will better designate the science of the study of the individual.

One might retort that words have a way of changing their meanings with the change of times and that the only requirement of a definition is that one use it consistently throughout a discourse. Such a view is taken concerning the present confusion in the use of "attitude" in the writing of Faris. "The question of definition and the inconsistency in the use of the word 'attitude' is . . . more a matter of lexicography than of science. A word means what men mean by it, and most dictionaries patiently record all the uses of the words in the language. If one author is inconsistent, and most of them do slip, he should be held accountable for the fault, but scientific progress will not be made by mere voting about words. It is also a matter of common knowledge that other words are used instead of the word 'attitude' to denote the same thing, . . . To the tyro this is confusing; but if we think denotatively, we cannot go far wrong. Even the word attitude could be abandoned and a meaningless symbol substituted without loss. We could speak of the element x which is left as a residue of a former action and predisposes to a future act or type of acts."(1) Objections to the view of this quotation would be insignificant but it should be observed that 'attitude' is but one topic or name of many in a larger field while psychology is a reputed science subsuming the many topics.

Investigations of Book (2) and English (3) as to why students register for psychology give an insight as to the possibilities in academic presentation. Both investigators asked for written answers to questions such as (a) their real purpose in taking this course; and (b) what they expected to get out of the course. With

few exceptions the reasons were—"tautological, one should take psychology because it is a good course to take". Further as an answer to the confusion of definition the concluding paragraph of English (3) could not have been written more apropos. "As a more positive suggestion ought we not to take pretty seriously the fact that our students approach the study of psychology as nearly as possible without an intrinsic motive? We teach that learning is more effective when the learner has a specific goal in mind; but our students not only do not see the goal, they do not even know in what direction the targets lie. Surely, therefore, it should be one of our major tasks to show them just what are the legitimate objectives of the course and how the material presented subserves these objectives. The writer for one emphatically disclaims the belief that the true objectives are the narrowly utilitarian ones which alone the student seems to envisage. All the more it is our duty continuously to substitute real ones. If we fail to do this we lay ourselves open to the suspicion that we ourselves are not too clear as to why we teach what we do. And about that it is well to maintain a discrete silence."

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THE MENSTRUAL TABOO AND WOMAN'S PSYCHOLOGY

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ARE the ill effects of the menstrual period partly mental, as well as physical? Are they to some extent the result of social tradition, rather than of purely physiological origin? These are the questions which led to this survey of the history of menstrual taboos, in order to ascertain whether or not they possess a rational background, and to an experimental study by means of a questionnaire. The results are here briefly summarized.

The origins of menstrual taboos are usually ascribed to one of the following three causes: (1) the primitive's abhorrence for uncleanliness; (2) the protective reaction of the female, a phenomenon found among animals; and (3) the awe and fear of blood. Of these only the last is tenable, since the primitive does not have a universal distaste for uncleanliness, as is manifested in other of his actions. The biological theory is out of the question.[1] Thus the menstrual taboo, as is the case with many other taboos, does not spring from any rational cause, but is superstitious in origin.

The same is true for the rites and folklore which surround the menstrual period. In primitive societies, as well as in ancient, medieval and contemporary civilizations, many taboos and restrictions in regard to this period are found. The primitive taboos are no more irrational than the modern taboos, such as the one which forbids a woman to bathe during the catamenial period. The persistence of such folklore in the modern home is due to some extent to the ignorance of women in regard to the function of menstruation. The fact that rites and folklore contribute to the mysteriousness and "sacred" character of the menstrual function exercises some effect upon individual psychology. Here suggestibility seems to play a part.

Without postulating tradition as the sole cause for menstrual disorders, this paper seeks to discover whether it does not play some part in woman's attitude toward the menstrual period. For

[1] For various reasons, such as the dubiousness of analogies between animal and human behavior, the lack of need for such protection, and the popular notions concerning the likelihood of impregnation during the menstrual period, notions which should call forth "instincts of mating" rather than "instincts of protection".

this purpose a questionnaire was distributed among women attending a college summer session. It inquired into the mental and physical reactions of the subjects before, during and after the period.

The group used in this study was composed of 75 single women, all but one of whom were enrolled in college, and most of whom were graduate students. Their ages ranged from sixteen to forty-one, and the appearance of their first menstruation ranged from the ages of eleven to sixteen.

The range of intervals between the beginning of one period and the beginning of the next was from 20 days to 6 weeks, and the range of durations of the period was from 2 to 7 days. Thus the sample, though fairly homogeneous in social status, presents a great variety of physical differences.

Results of this questionnaire showed that 16 per cent went to bed during the period, while 4.3 per cent never found this necessary; 42.6 per cent sometimes went to bed during the period. Tables I and II were compiled from the answers to eight of the psychological reactions, the first table from the answers of those who went to bed periodically, the second from those who never found it necessary to go to bed. These groups were chosen because their answers presumably indicated more nearly than any others, the physical condition of the subject at the time of the menstrual period.

The answers of both groups tend in the same general direction. On the whole, the greater percentage of women in the No-Bed Group answered that there was no difference in any of their reactions during the menstrual period. Logically, however, it would seem that those women who enjoy greater health during the period would manifest fewer unfavorable psychological reactions. Whereas this does follow to some extent, the results of the questionnaire indicate that those women who do not go to bed are affected almost as adversely as those who do.

As a matter of fact, in four types of unfavorable psychological reactions, the No-Bed Group gave higher percentages of unfavorable reaction than the Bed Group. Again, a smaller per cent of the No-Bed Group said that they learned less before, concentrated better before and were more cheerful and active before menstruation, indicating by these answers that the menstrual period has a decidedly unfavorable reaction upon their mental activities.

When the trend of answers in regard to the mental and physical manifestations were compared, the preponderance of answers fell in the "no difference" and "no answer" columns. The

answers for the physical manifestations are more diffuse, *i.e.*, more nearly the same number of answers were placed under the different time headings: before, during and after. This would indicate that these manifestations are almost entirely individual, rather than common to women as a whole. On the other hand, the

TABLE I

12 WOMEN WHO GO TO BED PERIODICALLY *—BED GROUP

	Before	%	During	%	After	%	No difference	%	No answer	%
Temper	5	41.6	3	25.0	0	0.0	1	8.5	3	25.0
Cheerful	1	8.5	0	0.0	5	41.6	3	25.0	3	25.0
Active	1	8.5	0	0.0	5	41.6	2	16.6	4	33.3
Learn more	1	8.5	0	0.0	2	16.6	5	41.6	4	33.3
Nervous	6	50.0	3	25.0	1	8.5	0	0.0	3	25.0
Depressed	6	50.0	3	25.0	1	8.5	0	0.0	3	25.0
Concentrate better	1	8.5	1	8.5	6	50.0	3	25.0	3	25.0
Think more slowly	2	16.6	5	41.6	0	0.0	2	16.6	3	25.0

* Some answers were duplicated.

answers under the psychological reactions were more positive, indicating that women may be influenced by tradition; physical disability should accompany psychical disability if the physical condition were directly responsible for the psychological condition. When they are not closely correlated, they probably do not stand in the relationship of cause and effect.

The general consensus of opinion falls more largely under two heads, nervousness and depression, than any other. Only 8 women

TABLE II

31 WOMEN WHO DO NOT GO TO BED DURING THE PERIOD *—NO-BED GROUP

	Before	%	During	%	After	%	No difference	%	No answer	%
Temper	12	38.7	5	16.1	1	3.3	10	32.2	5	16.1
Cheerful	0	0.0	0	0.0	9	29.0	13	41.9	9	29.0
Active	2	6.4	3	9.6	10	32.2	10	32.2	7	22.5
Learn more	0	0.0	1	3.2	8	25.8	10	32.2	12	38.7
Nervous	15	48.3	16	51.6	1	3.2	5	16.1	2	6.4
Depressed	12	38.7	13	41.9	0	0.0	7	22.5	5	16.1
Concentrate better	1	3.2	0	0.0	12	38.7	10	32.2	8	25.8
Think more slowly	6	19.3	11	35.4	0	0.0	11	35.4	6	19.3

* Some answers were duplicated.

stated that there was no difference in nervousness, 3 failed to answer, whereas 41 and 35 said that they were more nervous before and during, respectively. The figures for depression (37 before, 29 during, 13 no difference, and 6 no answer) also show that women are affected unfavorably by the menstrual period.

Nevertheless, the fact that the figures for "before" and "during" are so close in both cases shows that there is a great amount of individual variation. If the majority of women were affected at some one definite time in relation to the period, more positive conclusions might be drawn.

Although the results of this questionnaire can do very little to establish positive evidence of detrimental effects or the absence of such effects of menstruation upon women, they can, to some extent, show the effects of tradition upon the feminine mind. There is also evidence that women have not observed very closely the effects of the menstrual period upon themselves. This factor in itself points to the conclusion that its effects are in part mental and traditional rather than biological, that women are still under the spell of the primitive taboo. This paper does not intend to discount the biological factors, but to point out the possibility of the strong effects of superstition and taboo upon the menstrual period, and to lay a foundation for further inquiry along the same line.

THE INTELLIGENCE AND ACHIEVEMENT OF MEXICAN CHILDREN IN THE UNITED STATES

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PREVIOUS studies of Mexican children in the United States have provided results concerned with intelligence test scores with little cognizance taken of the influence of educational achievement upon those results. To date, in so far as we are informed, the only relation noted has been the influence of grade placement upon the intelligence score. Here it was found that this influence is moderately strong. There is, according to Garth, a correlation between mental test scores and school grade of .69 when age is controlled.(1) While Garretson claims that the chief cause of poor performance of Mexican children in the United States is inferior mental ability, he does not indicate the measured influence of educational achievement on the measured mental ability.(2) It is necessary to seek some measure of this influence and that is the chief purpose of this study.

THE SUBJECTS AND THE TESTS

The subjects of this study were 683 Mexican children, 440 of whom were from the El Paso, Texas, public schools, and 243 were from small towns and rural schools in New Mexico, one of the former being a Catholic school. The boys and girls were about equally divided, their numbers being, respectively, 338 and 345. The ages range from six to eighteen years and the grades from the fourth to ninth grade. The majority of these children come from homes where the standard of living is low and shiftlessness prevails. Many of the homes are of adobe brick. Inside these are found no beds and they have only dirt floors.

COMPOSITION OF GROUP

	Grade						
	4	5	6	7	8	9	All Grades
Boys	22	92	143	61	8	12	338
Girls	29	118	106	59	17	16	345
Total	51	210	249	120	25	28	683
El Paso	440 cases						
Other Schools	243 "						

In El Paso the Mexican children are segregated from the whites, while in the other schools where the testing was done about 10 per cent of the students enrolled were American white children. In the schools visited few of the Mexican children are able to speak English when they start to school, although the ancestors of many have lived within the United States for three generations. However, among these children there were some who came from more prosperous homes and had better educational home surroundings. Not only was this a fact, but it was well indicated by neatness of dress and refinement in behavior.

The test administered to these subjects was the Otis Classification Test, which is composed of an educational test (Part I), and an intelligence test (Part II). A score in Part I measures the subjects' ability in various school subjects combined which gives an idea of educational advancement in general, but in no particular subject. The intelligence score is just about the same as is found in the well known Otis Intelligence Test score.(3)

PROBLEM

Here the immediate problem is to find (1) What is the influence, as measured, of educational achievement upon the intelligence score of these children? Other questions may to a certain extent be answered by handling this data. These are: (2) What is the indicated mental and educational growth of the Mexican children? (3) What is their educational retardation? (4) What is their mental age for a school grade? (5) What is the educational age for the same? (6) What can we ascertain regarding the group I.Q. and the E.Q. and lastly the A.R., *i.e.*, the educational quotient and the accomplishment ratios.

HANDLING THE DATA

To handle the data for separate school systems was impractical because the total number was too small for too much division. While the main purpose of the study is to answer question (1) indicated in the foregoing "Problem", it seems better to answer all the other questions before answering that for clarity of answer to (1) depends upon the others for background.

On this account, Table I contains such facts as show corresponding to the chronological age the achievement and intelligence score for the Mexican children and the norms for whites, also the overlapping of the distributions of the Mexicans on the American white norms. We have obtained these white norms from the Otis Manual.

Table II supplies the median chronological age for school grade and for the total group, median test scores for intelligence and achievement, median mental age for grade and total; likewise educational age in a group, I.Q. and E.Q. and A.R. Figure 1 supplies in graphic form some of the tabulated facts which enable the reader to better follow the interpretation.

TABLE I

SHOWING MEDIANS FOR SCORES IN ACHIEVEMENT AND INTELLIGENCE FOR EACH AGE FOR MEXICAN CHILDREN AND NORMS FOR AMERICAN WHITES, ALSO OVERLAPPING OF MEXICANS

		Mexican				White American			
Age Yrs.	No.	Score Med.	Achiev. Q.	Score Med.	Intel. Q.	Achiev. Norm	Mex. Overlap %	Intel. Norm	Mex. Overlap %
18.5	5								
17.5	21	41	14	33	12.3	70	3.9	58	5.0
16.5	55	45	6.3	30.4	12.2	67	14.9	56	12.9
15.5	98	45.8	13.5	31.1	10.5	63	15.5	53	8.1
14.5	151	45.1	7.4	34.7	9.5	59	19.9	49	14.9
13.5	156	42.1	10.0	27.7	9.6	53	25.3	44	26.4
12.5	98	38.5	10.7	26.9	7.2	45	35.9	38	17.3
11.5	62	31.7	10.2	22.1	9.8	36	43.3	31	30.6
10.5	27	20.5	5.5	13.5	11.3	26	33.3	23	22.2
9.5	9								
8.5	0								
7.5	0								
6.5	1								
Total	683								

In Table III is found the obtained correlations between the four variables, as (1) intelligence measure (score Test II), (2) grade placement, (3) educational achievement (score Test I), and (4) age. Following these are found the coefficients of partial correlation and a coefficient of multiple correlation along with a regression equation from which we obtained the relative weights of the several factors.

INTERPRETATION

Upon examination of Table I and Figure 1, it will be seen that in both achievement and intelligence score these Mexican children while starting out much like the American whites became less and less like them from age to age, ending up with a wide disparity. The overlapping assists one in seeing that this is quite true for they start with 33 per cent and 22 per cent for achievement and intelligence respectively, and end with 3.9 and 5.0 per cent respectively. Thus the performances go from similarity to dissimilarity. In intelligence, the line for the Mexicans is seen to show a

disposition to climb until the fourteenth year is reached when it shows a tendency to be on a dead level. This is likewise true of the achievement line. However, in the seventeenth year there is a drop. It should be noted that the numbers supporting the tenth and seventeenth years are rather too small for trustworthy inferences.

TABLE II

SHOWING CHRONOLOGICAL AGE, MENTAL AGE, EDUCATIONAL AGE, I.Q., E.Q., AND A.R. FOR THE MEXICAN CHILDREN FOR A GRADE AND OVERLAPPING FOR A GROUP, ALSO RETARDATION FOR MEXICANS

Grade	4	5	6	7	8	9		Overlap on White Norm %
Number	51	210	249	120	25	28	Total 683	
Chronological Age								
Med.	10.9	13.3	14.1	14.7	15.8	16.1		
Ave.	11.1	13.3	14.1	14.7	15.9	16.2		
S.D.	1.4	1.5	1.4	1.3	1.4	1.3		
Mental Age								
Med.	8.3	9.9	10.9	12.7	12.5	12.6		
Ave.	7.6	10.2	11.2	12.6	12.5	12.9		
S.D.	1.3	1.3	1.8	2.5	2.3	2.1		
Educational Age								
Med.	9.2	10.7	11.8	13.8	13.5	12.6		
Ave.	9.1	10.6	12.0	13.7	13.4	13.4		
S.D.	1.1	1.4	1.5	2.0	2.3	2.6		
I.Q. Otis								
Med.	73.6	78.7	82.7	92.2	87.5	85.7	83.2	11.7
Ave.	81.6	78.5	87.6	90.0	77.0	86.8		
S.D.	14.0	14.1	13.2	15.9	12.9	13.5		
I.Q. Terman								
Med	75.7	76.6	78.9	87.1	79.0	81.1	79.6	11.4
Ave.	77.9	76.7	80.2	87.8	77.0	87.9		
S.D.	15.2	14.7	14.3	19.2	17.2	14.9		
E.Q.								
Med.	83.9	80.1	85.3	95.3	83.3	82.9	85.5	18.9
Ave.	80.0	82.5	88.1	94.9	78.0	86.1		
S.D.	17.0	16.6	13.4	17.5	21.1	16.5		
A.R.								
Med.	112.2	107.3	107.7	107.4	109.3	101.3	107.5	79.3
Ave.	109.5	107.6	107.4	107.6	107.4	107.1		
S.D.	10.3	9.4	9.7	10.4	12.5	10.8		
Per cent of Retardation	47	80	79	74	76	57	Average Retardation 68.8	

It will be seen again upon examination of Table II that these Mexican children are much retarded educationally for the chronological age for a grade is much greater than that for the average white child of the United States. On the average the retardation is here shown to be 68.8 per cent for the total group. In the fourth and fifth grades, it is respectively 47 per cent and 80 per cent. If we note the measures indicating medians of mental age—obtained from intelligence scores, these are below the mental age for a grade

for the American white children, and much below the corresponding chronological ages for the Mexicans. The relation is best seen by examining medians for the I.Q. for a grade. While the educational age for a grade is seen to be below that of the American whites, it is slightly above the mental age for a grade generally.

For the total group of 683, the Otis I.Q. (calculated according to the Otis Method) and the Terman I.Q. (according to Terman's

TABLE III

SHOWING VARIOUS CORRELATIONS

Variables: (1) Int. Sc., (2) School Grade, (3) Ach. Sc., (4) Age, (P.E.) Coefficients and probable error.

	(1)	(2)	(3)	(4)
(1)		.56	.80	.25
P.E.		.02	.01	.02
(2)			.56	.55
P.E.			.02	.02
(3)				.29
P.E.				.02

$r_{12.4} = .52 \quad r_{13.4} = .76 \quad r_{23.4} = .50$

Taking $r_{12.4}$ as r_{12}, $r_{13.4}$ as r_{18}, $r_{23.4}$ as r_{23}

We have $r_{12.3} = .25 \quad r_{13.2} = .67 \quad r_{23.1} = .19$

$x_1 - b_{12.3} x_2 + b_{13.2} x_3 = .1775 + .6566$

Relative weights, school grade and achievement score = 1 and 3.7

$R_{1\,(23)} = .58$

Method) are slightly different, the median for the former being 83.02 and for the latter 79.6. This latter I.Q. is about what is usually found for Mexican children in the United States when verbal tests are used. There is a tendency on the whole for the I.Q. to rise from grade to grade. The E.Q., with some exceptions, is more disposed to remain constant. It is interesting to note that the median E.Q. for the group is 85.5, and is higher than the median I.Q.'s. As to the accomplishment ratio, it is in every grade higher than is usually found for the average American white, which is 100. The median for the total group is 107.5 with an overlapping of 79.3 per cent on the white norm. A more convincing handling of the data is found in Table III. Here the coefficient of correlation between intelligence and school grade is only .56 and between intelligence and achievement is .80, which

suggests that the grading is influenced not altogether by intelligence but that achievement is more influenced thereby. School grade and achievement do not have the correlation one might expect. It is only .56, just about the same as school grade and age, .55. Age and achievement have a rather low correlation, .29.

Since age in such studies is a factor which may well be controlled, we have controlled it in a further study of relation between the effects of achievement on intelligence as measured.

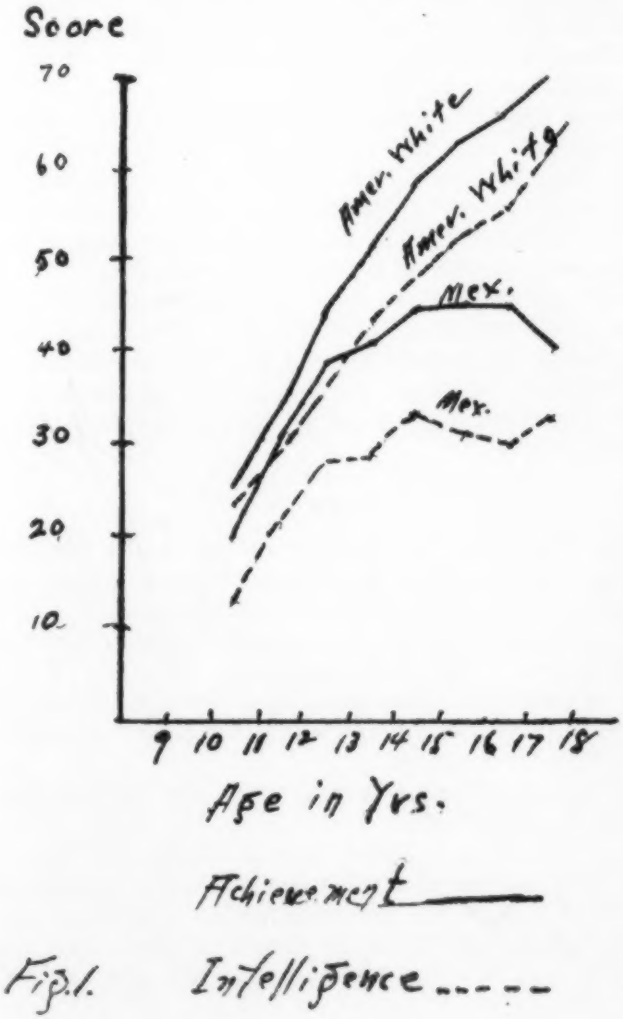

Fig. 1.

We have then by elimination of age between intelligence and school grade a correlate of .52 which shows that age had a slight influence only. Between intelligence and achievement, we have a correlate of .76 and between school grade and achievement a correlate of .50. In fact, the effect of age is slight, but in the same direction in influencing all the factors considered. Eliminating age, we have with achievement controlled the influence of school grade in intelligence as only slight, *i.e.*, a correlation of only .25. But the influence of achievement score is quite evident when we control school grade, *i.e.*, .67. If we eliminate intelligence and find the influence of school grade on achievement, we are not surprised to find it so low, .19.

One notes upon examination of these correlation coefficients that achievement and intelligence are fairly highly correlated. In order to measure the combined and relative influence of school grade and achievement on intelligence score, we have obtained a multiple correlation between intelligence score and school grade and achievement as well as a regression equation.

The multiple correlation coefficient between intelligence score and school grade and achievement is .58. The relative weights of school grade and achievement are in the ratio of 1 to 3.7. That is, while the influence of school grade and achievement is a real fact, achievement is about four times as strong as school placement in its influence.

SUMMARY

1. These data show the Mexican child as more like the American white at the early ages, but less like them as they grow older in both achievement and intelligence as measured.

2. The chronological age for a grade is high and the educational retardation is high (68.8 per cent).

3. The mental and educational ages for a grade are below the American white norms, but the educational age is slightly above the mental age for a grade.

4. The Otis and Terman group I.Q.'s are for the total group, respectively 83.02 and 79.6.

5. The median E.Q. is higher than in the median I.Q.

6. The A.R. indicates that on the whole the ratio between the I.Q. and the E.Q. is higher than it is for the American white.

7. The highest correlation is found between intelligence and achievement, *i.e.*, .80; the next between intelligence and school grade, *i.e.*, .56.

8. A multiple correlation was found between intelligence and

the factors, school placement and educational achievement. It is .58.

9. The relative weights assigned to school placement and achievement are 1 and 3.7, showing that the latter is the more influential in obtaining an intelligence score.

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REVIEWS

THE NATURE OF HUMAN CONFLICTS, OR EMOTION, CONFLICT AND WILL: AN OBJECTIVE STUDY OF DISORGANIZATION AND CONTROL OF HUMAN BEHAVIOR. By A. R. Luria, Professor of Psychology at the Academy of Communistic Education; Research Associate, State Institute of Experimental Psychology, Moscow. Translated from the Russian and edited by W. Horsley Gantt; with a Foreword by Adolf Meyer. New York: Liveright, 1932. Pp. xvii + 431. $4.00.

"Mankind will possess incalculable advantages and extraordinary control over human behavior when the scientific investigator will be able to subject his fellow men to the same external analysis as he employs for any natural object." With this statement by Pavlov, Luria evidently agrees. He is however neither a behaviorist nor a psychoanalyst, but a Marxian psychologist. He holds that human behavior cannot be explained by elementary dynamics; elementary neurodynamics can be understood only in relation to our "culturally created psychological functions", as part of "a dynamic system" (7). He rejects accordingly the Pavlovian emphasis on elementary excitation and inhibition, and thinks of organization and disorganization. Organization of adult behavior means, for Luria, a development which dominates and transforms primitive behavior; and disorganization is a disturbance in the total behavior of the animal (9, 11–12).

To understand "the whole disorganization of behavior", also the organization of behavior, Luria considers the motor system as inclusive of the central activities (17, 23). He studies particularly "the involuntary destruction of the voluntary movements"; voluntary movements being demonstrably "not less regular nor less stable than the reflexes" (20, 21). When, for example, Luria asks a subject to respond to a stimulus word both linguistically, telling the first thought that comes into his mind, and manually, pressing a receiver which registers graphically, we have "a very objective means for drawing conclusions concerning the structure of the internal neurodynamic process" (24). This technique reveals inhibited speech tendencies, also complexes, emotional states, and efforts at deception (31ff.). Given conflict of any sort in this situation, the subject's motor responses are quick, diffuse, and varied. Individual differences, also, appear. Of students thus tested before an examination, about 25 per cent, apparently whether or not well prepared for the examination, were remarkably free from the "abnormal" reactions just cited, and were put down as "reactive-stable" individuals; while about 30 per cent, who also showed neuropathic symptoms, were "reactive-labile" (73–76, 292).

The latter group, Luria suggests, suffered destruction of that "functional barrier" between excitation and motor response which enables the

normal adult to check his motor responses until his elaborative, connective processes have mobilized "accurately the amount of excitation required by the situation" (289, 342). Such destruction Luria explains by weakness of the higher cortical mechanisms or strength of subcortical mechanisms (370). In pseudobulbar paralysis and in the lighter forms of Parkinson's disease, the "functional barrier" is abnormally strong, and the sufferers' behavior is correspondingly "stable" (293–294). In the psychoneuroses, on the other hand, also in aphasia, in feeble-mindedness, in early childhood, in adult fatigue, in diversion of attention, and in the use of an unaccustomed hand, "every excitation, even in the beginning of its stages, is directly transferred to the motor system": the behavior is extremely "labile" (294–295, 338ff., 379, 385, 386, 391). Thus the responses of the young child, in experiments on tapping, delayed reactions, and choice reactions, are diffuse, impulsive, and roughly proportionate to the stimulus (334, 373, 343, 346).

All these observations lead to the concept of "the neurodynamical age" of the individual; a concept which remains to be worked out quantitatively, but which must be as useful a measure, Luria thinks, as the morphological age and the mental age already in vogue. Certainly Luria's researches show individual differences in resistance to behavioral disorganization, and different levels of behavior at which the disorganization may occur. Some neurotics cannot give rhythmical pressures; others give the pressures but break down at "simple reactions"; others, again, are incapable of "delayed movements"; and still others become erratic only when speech is combined with hand reactions, or when some further central complication is added to the experiment (362, 302ff.).

Luria shows further that efforts by the subject to control his reactions "directly" in these experiments fail, whereas "indirect" methods, using rhythm, auxiliary signals, or speech, or including the given activity in a "definite structure" or task (Lewin), often succeed, even in hysteria or *paralysis agitans* (409ff.). Obviously these findings throw much light on aphasia (270ff.), and on psychotherapy. Indeed, intellection as a means of control seems to be just such ancillary activity; which explains the "functional barrier" (409ff., 420–423). Since the reactive processes of the human being "are constructed not only from *below* out of the simplest neurodynamic mechanisms, but also from *above*", out of "the higher symbolic mechanisms" and regulating systems, particularly speech, Luria concludes that the "functional barrier" is of cultural origin. "Speech and the use of signs . . . permeate the whole activity and structure of behavior so that we find them literally in every movement of the fingers (387–389, 395–396, 428).

Along with the contributions so far mentioned, Luria demonstrates, experimentally, conflict in word-association tests; conflict between hypnotically suggested activities and waking activities, etc. (205ff., 254ff.); emotional upsets from conflict; negativism, likewise (262); partial and compromise responses (261); symbolic, approximating, responses (263); the rise of "suppressed complexes" in free association (245, *circa*), with

conflicts proportionate to the personality's antagonism to each complex (248, 165–166); "repression" of complexes and "resistance" to their recall (149, 154, 155, 159–160); stereotypies (263); substitution of stereotyped verbal reactions for "revealing" motor reactions (122–127); neurotic perseveration (320); "flight into reality" (263); "expression" of a complex as the expressing medium, say the hand, is included in that complex's system (173); type of expression according to type of individual (176); what the reviewer has called "alternative response" (188ff., 325); the adjustive use of symbols (425–426); and the relief of conflict through "connective, coupling-up, . . . reconstructive" activity (264).

Unfortunately, this mass of valuable material is not well presented. Luria's psychological theory seems, if not so naïve as Pavlov's, yet naïve and confused. Modest about his hypotheses, in places, many statements nevertheless sound very general and final. His accounts of experimentation are sometimes puzzling: "We arranged all of our experiments in such a way that the undifferentiated word-stimuli were of equal value for the subject" (99). He criticizes psychologists for "localizing" functions; but he speaks of "receptory, connector, and motor conflicts" respectively. He sees connections between religious confession and psychoanalysis, but he fails to analyze the conception of "catharsis", and does not trace any connection between "catharsis" and "countermanding a suggestion" (114–122, 131). His uses of the term "conflict" give pause. His diagrams and tables of data are not uniform and adequately explained, and contain arithmetical and other errors ("A" and "B" on p. 29 are reversed).

The style of the book is jargonish (Quiller-Couch), repetitious, unclear: "The development of the child results not only in the inhibition of the primitive forms of activity of the nervous system; it proceeds along the path of the general development of regulation which begins with the primitive aspects of instinctive capabilities, and, with the help of the development of the higher psychological mechanisms, it approaches the most complicated forms of control of its behavior" (10). . . . "Similar experiments in dogs in more acute forms have been demonstrated by us in the cases of inhibited compulsion, where the subject collides with the marked obstruction during direct trials to establish the compulsive speech reaction" (268). Terms like "neurodynamic process", "excitation", "excitatory", "intensity", "mechanical", "mechanism", "mechanization", "organic", "displacement", "settings of the personality", "inadequate", "definite", "precisely", and "completely" are used very loosely, curiously, or unclearly. For "replaced by" we find "substituted by" (364). Typographical errors, also, are not lacking.

If further psychological contributions are to come, as we hope, from the new Russia, we should ask that they be adequately written.

Smith College. W. S. Taylor.

THE HUMAN PERSONALITY. Louis Berg, M.D. New York: Prentice-Hall, Inc., 1933. Pp. xv + 321. Price, $3.00.

In spite of the book being marred in a few places by uncritical statements, it should be of value to teachers, parents, and social workers, who wish a brief account of the various conditions that influence personality and behavior. In the preface the author criticizes the narrow viewpoint of most specialists who exaggerate the importance of the findings in their own field and minimize the importance of the findings in other fields. To correct such one-sided emphasis is the author's purpose. He does this nicely.

The author begins with an account of the biological and physiological bases of behavior and personality. He then shows how environmental conditions influence the development of personality. By means of well selected cases the point is driven home that personality is a product of heredity and environment. Especially valuable are the following chapters: The Glandular Basis of Personality, Maladjusted Personalities, Personalities in Conflict, The Criminal Personality, and The Psychoses: Personalities in Flight.

There is no bibliography, and only a few references to the work of others.

University of South Dakota. CHARLES C. JOSEY.

CHILD PSYCHOLOGY. By A. T. Jersild. New York: Prentice-Hall, 1933. Pp. xiii + 462.

Any elementary textbook in Child Psychology seems to suffer from one of two difficulties: the material either represents a selective prejudice, or appears to be disjointed because of the attempt to be inclusive rather than penetrating. In the present case, the latter fault seems apparent to the reviewer.

In general, the scheme of the book suggests the fourfold categories of normal development used by Gesell. After two hasty survey chapters on the neonate and infant sensorimotor norms, chapters on language, emotional, and social development follow. This disposes of three of Gesell's groups in order. At this point a chapter on learning (its nature and eleven experimental studies of the process) seems to be interjected to pave the way for the fourth and most intricate group, the adaptive behavior of the child. Then comes an outline of the "Growth of Understanding" (perception, reasoning, beliefs, and imagination). The impression is strong that the adaptive nature of these processes is not adequately treated. The other three Gesell groups seem much more fully developed, but these are the groups of least importance in a practical sense however interesting they may be from a normative point of view. The final chapters point to individual differences, to personality and character, and to various applied phases which seem out of place and somewhat of an after-thought.

By way of general summary, the book impresses the reviewer as poorly knit together and spread out too thin. In addition, the type-face contrasts

unfavorably (to reënforce this statement) with the average text where the same amount of material would have been compressed into half as many pages. Its essential virtue lies in its comprehensiveness, but this seems to be achieved at the expense of coherence and depth of understanding.

Dartmouth College. C. N. ALLEN.

HEREDITY AND ENVIRONMENT. G. C. Schwesinger. New York: Macmillan, 1933. Pp. xi + 232.

The author's avowed purpose in this volume is the appraisal of the present status of psychological knowledge relevant to eugenic research. The first two chapters summarize the work in intelligence measurement and in personality measurement (bibliographies of 202 and 203 titles). The major chapter, on the influence of environment on intelligence, includes twin studies, foster-child studies, and the various examinations of culture factors and of physical factors in their bearing on intelligence (bibliography of 229 titles). A fourth chapter, differing in character from the others, presents the behaviorist, Gestalt, psychoanalytic, eidetic, and other points of view of personality, and the contributions of these schools to the understanding of affective, emotional, attitudinal, and volitional traits.

The compilations of studies, necessarily incomplete, are of little value except for the excellent critical comments which they have elicited from the author. She rightly insists that any discussion of the relative importance of heredity and environment is futile:

(1) Nature and nurture are not fixed entities with respect to each other. Both a eugenic elimination of feeblemindedness[1] and a dysgenic low p. 240 ff.)
birth-rate among the intellectuals would tend, by lessening the variability of nature, to enhance the importance of nurture; as would also an increase in the range of social and educational opportunities. On the other hand, educational and social equalization, by reducing the variability of nurture, would magnify the significance of nature.

(2) The influence of each environmental factor varies with different traits in different individuals.

(3) The tetrad-difference and other modern statistical techniques are just beginning to disentangle for measurement the individual and group factors of 'general intelligence'. This analysis the author believes most promising and significant to the eugenist.

Since to date no one has been able to study these disentangled specific traits of different human beings in varying specific environments—to say nothing of considering the fluidity of populations and of culture influences—and to say nothing, likewise, of the part which each trait plays in the total configuration of an act of behavior—one must conclude that the studies here reported are but the merest suggestions of the respective rôles of heredity and environment in the fascinating stupidities and scintillations of real human behavior, and that these studies have, therefore, a

[1] The eugenic program for eliminating the feebleminded, Jennings seems to regard as a biological whimsy. (Jennings, H. S.: The Biological Basis of Human Nature,

negligible eugenic significance. Perhaps it is well for us to be reminded now and then that like most of our fellow scientists we still see through a glass, darkly.

Dartmouth College. CHARLES LEONARD STONE.

PSYCHOANALYSIS AND MEDICINE. By Karin Stephen. New York: Macmillan Company; Cambridge: The University Press. Pp. 238. Price, $2.50.

Dr. Stephen has written an excellent book on psychoanalysis. Based on a series of lectures given at Cambridge, mainly to medical students, it presents the Freudian interpretation of "the wish to fall ill" in eight compact, logical, and admirably clear chapters.

The author brings to her task not only a first-hand knowledge of psychoanalysis, both practical and theoretical, but also a thorough appreciation of the ways of thought that characterize the natural sciences in the more conventional sense of the term—even of those ways of thought that some of the members of her profession are likely to dismiss as merely academic. Nowhere does she show the slightest impatience with the demands of ordinary science for ordinary logic, literalness, and intelligibility. It is especially noteworthy that nowhere does she shirk a problem by saying that the modes of unconscious thought are so strange and devious that it would be useless to attempt to explain them to the uninitiated. Instead, she goes straight at the task of accounting for the special strangeness under consideration in terms of particular experiences and concrete situations. It is significant that in doing so she uses a minimum of the picturesque and figurative terminology that has become associated with the psychoanalytic theory. She finds it entirely possible to say what she has to say in plain English, and in adopting this practice she cuts herself off from the possibility of leaving gaps in her reasoning which can be concealed or at least veiled by conveniently vague terms. Dr. Stephen's writing is concise, clear, undecorated, and direct; it is entirely lacking in the Viennese voluminousness of phrase that characterizes so many treatises on psychoanalysis.

The lucidity of the presentation, however, goes deeper than terminology. It is the product of the kind of sophistication which has achieved simplicity and clarity, and which shows its possessor to be so thoroughly familiar both with the possibilities and the limitations of common sense and of scientific knowledge as to be neither too scornful to use them nor too worshipful to be overawed by them. The result is a presentation of the principles of psychoanalysis which commands the respect even of a reader who disagrees frequently with the views presented. Even in the treatment of those aspects of the theory which, to a reader with academic leanings, seem most far-fetched in interpretation and most lacking in objective proof, the evident competence with which the author handles her material stamps the work as a balanced and well-considered exposition of a significant point of view.

It is to be regretted that in a book of this quality no reference is made

to the fact that there is a science of psychology which, by means of laboratory experiments, is studying such processes as memory, habit-formation and emotion, and which is gradually accumulating material on these and other topics relevant to the problems of psychoanalysis, which any student of human nature might find useful. But perhaps one has no right to criticize an author for not transcending the limits of a self-imposed task. After all, Dr. Stephen has chosen to write on psychoanalysis. Still, one cannot help reflecting on the wastefulness of a situation in which two lines of approach to an understanding of a common object have established so little communication with each other that it is possible to write on their common subject-matter from the one standpoint, without reference to the findings of workers who are studying the same material from the other point of view.

Wellesley College EDNA HEIDBREDER.

THE EUGENIC PREDICAMENT. By S. J. Holmes. New York: Harcourt Brace, 1933. Pp. xi+232.

This book is timely—witness the Nazi determination to force eugenic sterilization on some half million citizens for the good of a better Germany. What are the facts about the eugenic program? Admit with the author that our society is dysgenic, and that any improvement comes but slowly, and that the best of all gifts is to be "well-born"—is the picture as pessimistic as it is here painted? And if it is that bad, what is to be and can be done to improve it? At least the author is not included by Landman (*Human Sterilization;* see review in this journal, volume XXVIII, page 335) as one of the extreme alarmists.

The bias of this book is to emphasize heredity over environment, as is seen in concluding that studies of identical twins favor eugenics and places it in a strong position before its opponents. The first part of the book states the biological "facts", gives data on the roll-call of the ill-born, and states the thesis that superior mentality is dominant to inferior; that normal mentality is dominant to feeblemindedness, epilepsy, insanity, and chorea—a position first stated by him in 1921. Then follow three chapters on the sources of the birth supply, the elimination of unfit by natural selection, and an excellent rebuttal to the critics of eugenics. It is the final chapter that will be read most carefully by psychologists—"What Can We Do About It?" Two methods are suggested. The first is to adopt a negative program of cutting off bad heredity: segregation, sterilization, and birth control. For each of these methods he has an objection: too expensive, slow and hard to draw the line, and possible extinction by having too few eugenic births. The alternative is a program of positive methods—the increase of good heredity by eugenics. But the predicament now becomes apparent: legislation is almost impossible in a democracy. For a clearer understanding of this point the reader is referred to Landman once more.

In summary, Holmes has written a book that should answer his intent—propaganda. In his final chapter he concludes that the greatest eugenic reform will come from removal of the penalties of parenthood, perhaps by

some system of bonuses for each healthy child. The task, at present, is to improve sexual selection by eugenic education. Hence this book. For cross-reference read Landman and the more recent *Woman in Soviet Russia*, by Halle.

Dartmouth College. C. N. Allen.

Personality, Many and One: An Essay in Individual Psychology. By J. W. Bridges. Boston: The Stratford Co., 1932. Pp. vi + 215. Price, $2.00.

Neither the title of this book nor the subtitle is adequately descriptive, since it is no more a treatise on hysteria than an Adlerian essay. On the contrary it is a study, as the preface indicates, concerning "various aspects and types of personality without moral evaluation or ascription of ulterior meaning" (p. i).

The author's definition of personality is general enough to include the entire psychological field, namely, "the totality of all physical and mental characteristics, traits and processes, and their interrelationships" (p. 4). The book stresses these interrelationships of processes. Trait is regarded in equally sweeping terms. "A trait", Bridges states, "is any relatively permanent distinguishing feature or characteristic whether mental or physical" (p. 17). The author organizes his concept of personality around the British classifications of cognition, affection and conation, which form the basis, respectively, for his treatment of intellect, temperament and character. His view of character tends to agree with that of Roback save for the latter's assumption of innate powers of inhibition. Bridges thinks that impulses and drives are subjected to habits of control and "develop as a result of experience, guidance and training" (p. 102). These controls he regards as conscious and distinguishes them from repression. Important to the organization of character is "the resolution of conflict between antagonistic impulses" (p. 113).

Some consideration is given to the psychological aspect of physique. The relationships of the mental and physical types such as those of Sigaud, MacAuliffe, Kretschmer and Berman are studied and evaluated. Notwithstanding the author's acceptance of the unimodal approach to typology, his longest chapter is devoted to "Psychological Types". He discusses in considerable detail the classifications of James, Jordon, Jung, Spranger, Rosanoff and others. Bridges seems particularly impressed with Jung's chief dichotomy of extraversion and introversion. Pregnant suggestions are made that these trends are determined by processes of sensation (p. 52), perception and attention (pp. 59, 60), thinking (p. 66), and feeling (p. 85),

A stimulating final chapter dealing with the degrees of "Development of Personality" contends for freedom of individual expression and responsibility. "The more impulses and the more control the richer and fuller the personality" (p. 189). External prohibitions he believes "impoverish and weaken character" (p. 190).

Accepting the double-aspect theory of mind and body, Bridges strives resolutely towards intellectual reconciliation. Favoring a broad associa-

tionism he nevertheless harmonizes such hostile factions as functionalism, Gestalt and behaviorism, including as well the views of Adler and Jung, to say nothing of the claims of neurology. His extraordinary capacity for clear presentation and logical organization lighten this difficult feat.

The book is not intended to be profound but it is forthright and closely knit together. The author is never carried away by the many alluring approaches to personality; his judgment is always sober, sensible and sound. Enlightened and interested readers, whether psychologists or not, will find the book helpful and worth while.

Dartmouth College. IRVING EDISON BENDER.

MOTION PICTURES AND YOUTH: A SUMMARY, by W. W. Charters, combined with GETTING IDEAS FROM THE MOVIES, by P. W. Holaday and G. D. Stoddard. New York: Macmillan, 1933. Pp. 66–102.

Getting Ideas from the Movies is one of the series of studies sponsored by the Payne Fund in the general survey of *Motion Pictures and Youth*. Holaday and Stoddard were assigned the problems of measuring the retention of the specific action and background and the general information shown in motion pictures. Like the majority of studies reported in the series, this one may be commended for the techniques which were devised to study the problem without distorting its original character. The investigators employed nearly 3,000 subjects, the procedure was well controlled and the results are unequivocal. The study shows that both children and adults do gather general information from motion pictures, children in the ninth and tenth grades increasing their information by about 40 per cent and superior adults by 28 per cent. If information is incorrectly presented, it is taken as valid unless the errors are clearly pointed out. We find that children in the second and third grades, when tested immediately, retain 60 per cent as much specific information as adults and that over a long period of time these children retain a larger percentage of the information gained than do adults. This finding, as well as many of those reported in other studies, suggests a relation between eidetic imagery and the effects of movies on children. It is regrettable that none of the studies provide the psychologist with any information on this point.

Perhaps the most significant psychological finding of the study is the fact that both children and adults remember best material that has a high emotional appeal, that is easily understood, and that is concerned with the plot of the picture. The retention of such material is shown to diminish but slightly over a period of three months. The contrast which the authors make of this result with the classical experiments on memory brings out the striking artificialities of studying learning in the laboratory with the use of meaningless material. From the point of view of learning as it occurs in everyday life, the findings of Ebbinghaus, Meumann, and Radossawljewitsch seem hopelessly inadequate.

It is impossible to review Dr. Charters' summary of all the Payne Fund Studies. The author has presented for the psychologist and educator a more technical outline of all the investigations than the popular summary

found in Forman's *Our Movie Made Children*. The effects of movies on learning, attitudes, emotion, and sleep, as well as the analyses of the contents of motion pictures, are restated.

Charters' book again raises a problem common to many of the investigations, namely, the relation of movies to anti-social conduct. The author implies that the movies are largely responsible for much undesirable conduct and concludes that "the simple obligation rests upon those producers who love children to find a way of making the motion picture a beautiful, fascinating, and kindly servant of childhood" (p. 63). The futility of this exhortation is apparent when it is compared to the current Hollywood evaluation of a "good" picture as one that brings large financial returns (*cf.* W. B. Pitkin and W. M. Marston, *The Art of Sound Pictures*). It is clear that the motion picture producers are more interested in profits than in the social elevation of the masses and if socially desirable pictures are ever to be made it appears that they will have to be produced by non-profit seeking organizations. This conclusion provides a further challenge to those who want better pictures.

There is obviously more than a direct causal relationship between bad movies and bad conduct. The importance of movies as determinants of anti-social behavior is not satisfactorily discussed in any of the studies. Dr. Charters states that "tendencies toward unapproved conduct and movie influence work together to produce more movie interest on the one hand and more anti-social conduct on the other" (p. 13). The appeals common to racy movies, stories, and conduct are no doubt due to personal maladjustments created by many factors in the social order itself and the popularity of undesirable movies might more realistically be regarded only as a reflection of underlying discontent.

Harvard University. HADLEY CANTRIL.

METHODS IN SOCIOLOGY: A CRITICAL STUDY. By Charles A. Ellwood. With an Introduction by Howard E. Jensen. Durham, North Carolina: Duke University Press, 1933. Pp. xxxiv + 214. Price, $1.50.

This volume should prove to be a valuable addition to the literature of sociological methodology. It is written with the clarity and effectiveness which one expects from Professor Ellwood. Of outstanding value is the depth of penetration into the concept of "method" itself. This is not a book on method in the sense of techniques of research, but rather an examination into the foundations of method—into the philosophical and psychological assumptions and concepts which underlie the use of specific techniques. The framework within which so much of American sociological thought moves can profit from such an examination. In this connection the author is inclined to agree with the European reproach that American sociologists as a group are lacking in broad scholarship: "There is too much truth in the European criticism that American colleges and universities train students in skills and techniques but do not develop scholars and, in general, favor superficiality and not the profundity shown by the best European thinkers" (p. 6). This may be painful to our self-

esteem but it will be worth pondering over. It leads Professor Ellwood to a plea for the recognition of the contribution of philosophical procedures in sociology; to an emphasis upon the historical approach; to a vigorous and incisive criticism of natural science (physical science) methods, objectivism and behaviorism in the social sciences, and a correlative defense of the method of "sympathetic introspection" or "critical imagination"; and to a critical discussion of the place of statistics and measurement in the field of sociology.

Perhaps the particular strength of the point of view and analysis of this study is to be found in its making the concept of culture of central importance. In the view of many, it is the rise of the cultural approach to a place of dominance in recent sociological thought and research which promises, if it has not already produced, a sociology which can rank as a self-respecting science. From this standpoint it is a little disconcerting for part of Professor Ellwood's argument to realize that one of the greatest contributors to the development of the cultural approach in sociology is also a champion of the statistical method, and is highly ingenious in applying the one to the other! One can agree with most of what Professor writes and yet feel that at this time the need is particularly great for the patient research which will prove or disprove hypotheses. In this respect measurement has a vital rôle to play, even if statistical generalizations are not universals but relative to a particular culture. Undoubtedly the direction in which to look is not one of theoretical speculation and logic versus fact accumulation and statistical manipulation, but rather some creative relation between the two, as implied in the reference to Pasteur: ". . . the history of the medical sciences shows many instances of imaginary hypotheses as to the cause of disease; but Pasteur with a few facts observed through the microscope, was able to overthrow all of these imaginary hypotheses, and to present for the first time a scientific theory as to the cause of many diseases, namely, the germ theory" (pp. 74–75). Statistical method may perform a function for sociology comparable to that of the microscope in biology; the case is still open. The negative attitude of Professor Ellwood toward some such possibility appears to be the most controversial issue raised by his otherwise judicious discussion.

Skidmore College. EVERETT V. STONEQUIST.

STUDIES IN THE PSYCHOLOGY OF ART. Under the direction of Norman C. Meier. Psychological Monographs, Vol. XLV, No. 1. Pp. 188. Princeton, N. J.: Psychological Review Company, 1934.

This volume inaugurates a new type of publication from the Psychological Laboratories of the University of Iowa. The results of research are compiled relating to the psychology of those phases of art activity directly related to painting and sculpture. They represent a continuation and an extension of the field already so ably considered by Professor Norman C. Meier, who has further supervised and directed these investigations. The intangible character of the subject matter, Applied Aesthetics,

could have been brought within practical and tangible grounds only by one gifted with extensive knowledge and experience.

This publication contains eleven studies, all except one dealing with some aspect of artistic capacity. Eight of the studies represent masters' theses, while three are doctorate or post-doctorate dissertations. The work has been facilitated by the coöperation of many agencies, the subjects used in the experiments have been, for the most part, children between the ages of two and ten years, and the assistance of the Iowa Child Welfare Research Station has been of immense advantage.

The first five studies deal with the consideration of artistic capacity as measured by aesthetic sensitivity through aesthetic qualities as principles of art. The subjects of the studies are as follows:

"Discrimination of Compositional Balance at the Pre-school Level." By Parmely Clark Daniels.

"The Sensitivity of Children of Pre-school Age to Rhythm in Graphic Form." By Constance C. Jasper.

"An Experimental Investigation of the Sensitivity of Children to Compositional Unity." By Katherine Snow Whorley.

"A Technique for Testing Color Harmony Sensitivity in Young Children." By Eileen Jackson Williams.

"The Sensitivity of Children and Adults to Color Harmony." By William E. Walton.

Professor Meier summarizes these experiments as follows: "The studies in their findings point to the conclusion that children not only do respond positively to the more fundamental aesthetic qualities or principles, but do so, as in the case of balance and rhythm, as early as the second or third year in certain individuals and by the fifth year in significant group averages. Unity and fitness show up in group averages several years later, but color harmony not until the twelfth year. These findings would tend to support the view that 'aesthetic intelligence' as an important element in artistic genius matures early in life."

The next group of studies makes use of carefully selected extreme types, the X group ranked as definitely superior and the Z group at the low end of the rating. This use of contrast is planned to show clearly the differences between the artistic and the non-artistic personality. The subjects of these studies are:

"A Study of Creative Artistic Imagination in Children by the Constant Contact Procedure." By Velma Bookhart Grippen.

"Playground Behavior Differentiating Artistic from Non-artistic Children." By Mildred Dow.

"Variation in the Aesthetic Environment of Artistic and Non-artistic Children." By Frances Rodgers.

"The Psychophysical Functions Differentiating Artistically Superior from Artistically Inferior Children." By Carolyn Tiebout.

It is the opinion of Professor Meier that these studies make possible most interesting observations but that further work is needed before larger

questions can be definitely answered. Certainly to the educator these preliminary findings are most provocative of thought. The art teacher will be particularly interested in the study by Grippen. The excellent illustrations of children's painting in color are a great contribution to the interest and clarity of this paper.

The two concluding dissertations present a slight variation. The first, "The Psychophysical Capacities and Abilities of College Art Students of High and Low Standing," by Hildegarde Fried Dreps, repeats under different circumstances and plan some phases of an earlier research by Manuel at Illinois.

The concluding investigation, "An Experimental Investigation of the Basic Aesthetic Factors in Costume Design," by Wilhelmina E. Jackson, presents three phases of the subject: first, a preliminary analysis of the factors in costume design; second, a determination of the basic factors of those accounting for beauty; third, an experimental determination of the underlying reasons for this fundamental character.

The monographs are finely organized and clearly written, while graphs and charts present statistical material in concise and clear form. Illustrations are generously supplied, a few of which are in full color. These add greatly to the clarity and interest of the text. Bibliographies are given at the end of each study.

It is indicated that these monographs are to be followed by others and that a program of research known as "Genetic Studies in Artistic Capacity," is to be developed. In view of the most interesting and informative material presented in the current issue, educators will await later reports with great interest.

Skidmore College. MARION D. PEASE.

DEMENTIA PRAECOX, *A Psychological Study*. By Harriet Babcock. Lancaster, Penna.: Science Press, 1933. Pp. 167. Price, $3.00.

The author demonstrates the advantage of studying dementia praecox otherwise than by subjective standards or from the vaguely speculative or metaphysical approach which emphasizes psychic trauma,—however useful from the therapy aspect. With the laboratory method, utilizing the quantitative tools of scientific psychology, Dr. Babcock furnishes valid estimates by which pathological functioning can be detected. By dissecting mental abilities and the thought process functionally she isolates the structural components in problem solving and cerebration in schizophrenia.

The psychogenic and physiogenic viewpoints of dementia praecox are first briefly discussed and the principles underlying normal mental functioning and mental impairment. The study seeks to ascertain whether dementia praecox patients differ from normal persons in the most elementary and basic mental processes such as reception and duration of sensory impressions, association processes, perception depending upon these processes with conscious recall, fixation based upon both associations and innate ability, recall and recognition also depending upon conscious and unconscious association and their degree of fixation, and responses whether

verbal or motor which require connecting associations before the idea can be carried into action.

Dr. Babcock uses the method for measuring mental impairment which she formulated and validated in *An experiment in the measurement of mental deterioration*, with paretics. Her premise, well authenticated experimentally, is that abstract intelligence may be exemplified by vocabulary which correlates highly with scholastic ability and intelligence generally and since it utilizes old learning, gives a fair measure of original capacity long after the onset of mental deterioration. She demonstrates that although abstract intelligence also has a high correlation with learning ability among normal persons, with psychotics there is a discrepancy between test results of these two abilities and that when mental functioning is impaired there is not only difficulty in concentrating on tests involving new data but also marked slowness in all stages of responses.

The general findings of this study may be summarized as follows: The mental efficiency of coöperative praecox patients as a group is inferior to that of normal persons in the understanding and execution of simple tests. The degree of inability of the praecox is directly related to the degree of measured inefficiency. Important as it is to regard the patient as a total functioning organism, of greater importance is it to consider the weakness which is within the total. The intellectual defect which Kraeplin considered present in the very earliest stages exists in measurable degree and his system of careful measurement and observation can with the aid of more clearly expressed psychological principles and better methods of measurement, be continued. The praecox inefficiency can only be attributed to physiological causes whose exact origin is unknown.

With mental level controlled the author finds that praecox patients learn more slowly than normals, which corroborates other studies; but contrary to frequent assertions, they do have an intellectual defect besides retarded mental processes, which affects the fixation stage of memory, causing fewer memories for meaningful material than normals retain. By differentiating the learning ability of the four praecox types and measuring the mental impairment of each, Dr. Babcock deems it possible to make an early diagnosis of the type which formerly could be determined only after long observation.

As to the course of mental deterioration in dementia praecox, the study shows the increasing lack of mental activity and general tendency toward increasing blankness of mind, explainable by slow associations which cause a fundamental incapacity to grasp ideas (dull apprehension), or to be subjectively stimulated by them,—which is often falsely mistaken for lack of attention and effort. The characteristic mental profile of a praecox with normal scores by other methods of examination, is abnormally poor learning but better ability, even normal with tests involving immediate repetition.

Comparing schizophrenics at different stages of deterioration with normal subjects, the slightly impaired evince normal or better duration of after-images which gives a superficial impression of mental clearness but which is united with a measurable slowness of functioning resulting in

defective perception and learning. Both phenomena are due to slow and relatively inactive associations which in immediate memory by not causing interference increase scores, but in learning by not furnishing associations to give meaning decrease scores. Further impairment is characterized by noticeably slow recall of well-known data and abnormal loss in psychomotor speed together with great increase in the earliest defects. With increasing deterioration, imperception of familiar words and sentences becomes exaggerated, accompanied by a failure of comprehension which precludes recognition of them even as familiar combinations of sounds and by an ever greater increase in previous losses. The most impaired cannot initiate even old habits at will though they might be impulsively performed with only chance momentary interest and controlled revival of previously learned responses as residuals. Periods of mental blankness are increasingly long and of mental clearness increasingly short, due to blocked associations.

Hart and Spearman concluded that praecox differences from normals were the same as differences found in stupidity but Dr. Babcock demonstrates that although the dull may learn less at first they gain more and with more consistency than praecox patients. In sum her evidence for organic defect is based upon similar impairment of perception and learning found in paretics and senile; upon normal or better ability in repetition tests and upon the regularity of changes, the course following the law of mental deterioration. The essential part of personality, the ability to associate present stimuli with past experience so as to understand and make controlled responses is gradually lost. Finally all ideas fail as cues to action or emotion with ensuing complete dementia.

The argument for the physiogenic basis of schizophrenia needs strengthening by extended research. Would the deterioration test administered to children of varying ages demonstrate mental impairment in schizoid types in line with the results obtained with adults? Would the same child of schizoid type finally become a praecox? Also how would psychoneurotics react to these situations? Can the deterioration tests assist in a truly differential diagnosis?

The physiogenic origin is upheld in this study even though environmental stress undoubtedly plays an important role in precipitating by devastating effect upon personality the visible course of schizophrenia. The author would contend that an early measurable psychological impairment could be uncovered by her tests. Although probably no amount of training can cure congenitally weak association processes, schizophrenics have been cured for all practical purposes. However the deterioration tests have disclosed impairment hidden to casual observation, even in but slightly dilapidated personalities.

Dr. Babcock's contribution rests not only in diagnosis but also in method and technique of measuring degrees of mental impairment. Thus an environment could be furnished which would not tax too greatly mildly pathological schizophrenics, as is provided for certain mental defectives. This suggests the possibility of removing some schizophrenics from prolonged incarceration in institutions and at a minimum risk to form a colony

such as that of Gheel in Belgium with a quasi-normal environment where episodic strain can be controlled to a great extent.

Probably ultimately it will be conceded that just as heredity and environment in effect are inseparables, acting as limiting or expanding forces upon one another, so the physiogenic and psychogenic viewpoints are reconcilable to the advantage of the schizophrenic.

This method of measurement is limited by the language factor and may therefore be inapplicable to the large proportion of foreign-born in state mental hospitals. For such cases there is need of another criterion of deterioration.

CLAIRETTE P. ARMSTRONG.

HYPNOSIS AND SUGGESTIBILITY. By Clark L. Hull. The Century Psychology Series, Edited by R. M. Elliott. New York-London: D. Appleton-Century Co., Inc., 1933. Pp. 405.

Most of the books published on hypnosis are the result of clinical work, and are intended for the consumption of psychotherapists. The contents of the books by Binet and Féré, Moll, Forel, Ringier, Janet, Prince, Bramwell, and Hollander bristle with "interesting" cases and numerous untested hypotheses.

A critical reading of the literature and a reinterpretation of the concepts in the field had been needed for some time. But more than that had been needed. What had seemed most necessary was quantitative facts, derived from controlled experiments, to replace qualitative logicalizing and sporadic case reports. Professor Hull, presenting the work completed by his graduate students and himself, at last supplies some long-felt needs.

A superficial reading of Hull's book would lead one to think that he is primarily eager to establish the superiority of conscious as against nonconscious behavior. A careful reading however leaves no doubt but that Hull refuses to dichotomize human behavior and insists that the human organism must be studied as a totality. Being a mechanist, Hull regards ideation as a verbal-symbolic act, and refuses to accede to neurologizing tautologies for an explanation of puzzling behavior facts. His approach must therefore be designated as organismic, objective, and experimental.

Mustering a great deal of experimental evidence, both direct and indirect, Hull shows that the factors generally associated with hypnosis are not necessarily associated with hypnosis alone. He shows that rapport, catalepsy, post-hypnotic suggestion, sleep-like behavior, hyperesthesia, and pathological dissociation are not essential characteristics of hypnosis. Rapport, Hull finds, is a special form of selective anesthesia, and is never encountered, unless suggested to the subject. Catalepsy, "falsely linking hypnosis with the tonic immobility of animals," can be induced as readily in the waking state as in a trance. Post-hypnotic suggestion, at least for commonplace action, seems to be less effective than ordinary waking instruction. Hypnosis is in no way related to sleep. If anything, it is the very antithesis of sleep, implying alert, rather than lethargic, response tendencies. Subjects in a trance believe that they have heightened sensi-

tivity, but this is by no means established. Such are some of the revelations contained in Hull's latest contribution to the science of psychology.

Defining hypnosis in terms of dissociation, Hull finds, is catering to an ancient dichotomy for which there is no justification. Hypnotic behavior for Hull consists in relatively heightened susceptibility to prestige suggestion, though he denies that mere susceptibility to prestige suggestion is hypnosis. Prestige suggestion is conceived as a habit manifestation which may be found in numerous other life-situations.

The experimental set-ups are excellent throughout. An attempt to have adequate controls was invariably made in each case. The conclusions are conservative, and Professor Hull appears to be his own best critic in each case, realizing the weaknesses, wherever present, and holding only to such inferences as could safely be made. Few indeed, therefore, are the points that a reviewer could bring up by way of criticism.

In many of the experiments performed the subjects were tested by means of apparatus which necessitated occasionally a condition bordering on immobility, and in others some degree of constraint. Without seeming to lend support to William James' timely attack on "brass instrument psychology", the reviewer wishes to note that it is doubtful whether, in experiments of this nature, such artificial conditions produce facts comparable to those found in normal waking life, or indeed even in the trance condition. Unquestionably subjects acting under the compulsion of a laboratory situation are already more restrained than they should be in order to yield life-like data. Adding the difficulties of apparatus cannot fail to magnify the chances of error.

In experiments on hypnosis, furthermore, there will always be a source of error inhering in the difficulty of obtaining comparable hypnotic phenomena. Hull used the lid-catalepsy index in some cases, and in others sought deep trance states by testing the subjects for post-hypnotic amnesia. Neither is entirely a reliable index of the depth of hypnotic condition. One has merely to glance through Davis and Husband's scoring system (Hull, p. 70) or the earlier description of "hypnotic stages" by Bernheim, to be convinced that there are quantitatively distinct types of hypnotic behavior, and that the two signs isolated by Hull give no assurance of uniformity of behavior in all the subjects employed. This indeed may account for some striking variations in tabulated data obtained by Hull in certain of his trance experiments.

Finally the reviewer wants to say a word about the total disregard of the psychoanalytic interpretation of hypnosis. Freud's contention that the hypnotic manifestation is an infantile regression in which the hypnotist plays the father rôle lends itself to investigation, and might have been included because of its various implications as regards the development of personality. Also, the types of individuals who are most adept at hypnotizing and the types most easily hypnotized might have been studied (as they have in Pavlov's laboratory recently), the two possibly in relation to each other. For it is well known that certain tpes of personality, too busily engaged in what Freud calls reality-testing, cannot be hypnotized,

at least satisfactorily, for obvious reasons. Also, it is known that those who cannot be hypnotized by some operators can easily be hypnotized by others. The variations in these respects seem to suggest fascinating topics for psychological study.

What might have been included is a question quite apart from our present discussion, since our task is to review what has been included. Insomuch as Hull has had to deal with a topic which is notoriously elusive and extraordinarily subtle, a literature reeking with inaccuracy and superstition, and a subject which is somewhat unpopular in psychological circles, he has done a significant piece of work. Few people have dared to go into the field at all, fewer still have dared to publish in it, and none—prior to Hull—has attempted to do a really scientific piece of work in it.

Representing many years' patient research, *Suggestibility and Hypnosis* is a tribute to the systematizing and organizing ability of Dr. Hull who has summed up in it what, without the aid of his brilliant students, would have been the work of a lifetime. This book, like similar works of Pavlov, Luria, and Dodge, to mention but a few recently published, is a model which psychologists should wish to follow. It shows clearly what a consistent program, in the hands of a research man thoroughly trained in scientific method, may bring to pass. Professor Hull must be congratulated on his timely and excellent contribution to the scientific literature of our day.

MAURICE H. KROUT.

THE APE AND THE CHILD. By W. N. Kellogg and L. A. Kellogg. New York: McGraw-Hill Book Co., Inc., and London: Whittlesey House, 1933. Pp. xiv + 341.

Twins reared in isolation have been held to test the effect of heredity; foster children reared in different families have been used to test the effect of environment; comparisons of foundlings and feral men with civilized men have been taken to be a test of either, or of both, heredity and environment. *The Ape and the Child* is a study of those variations in the behavior of organisms of different species which can be credited to phylogenetic evolutionary changes rather than to the influence of genetic factors.

A chimpanzee, whom the experimenters named Gua, was kept in the Kellogg home from the age of 2½ months to that of 16½ months. Donald Kellogg, a normal child, used as a control, was 5 months old when the ape was brought in, and was used for comparative purposes until the age of 19 months. There was no deliberate teaching of stunts or tricks. What was sought was responses to household situations, and not meaningless rituals elicited by prearranged signs. In this way the study here carried out differed from the work generally done in circuses and even from that of Pfungst, Köhler, and others who studied animal and human subjects.

Of course, tests were given from time to time, and some of them proved illuminating. Tests of speed of involuntary movements and of special reflexes, hand- and foot-preferences, grasping and prehension, perception, sensory responses, intelligence, and various types of learning behavior,

including a detour technique, a "Names" experiment, and a delayed reaction experiment, were employed by the Kelloggs. The results, together with a mass of other carefully recorded observations, are given under the captions of health, eating, and sleeping; dexterity, arm-movements, and walking; the senses; play; social and affectionate behavior; emotional behavior; learning; memory and recognition; intelligent behavior; and finally, communication and language.

The authors guarded against anthropomorphic pitfalls, and sought to do their work carefully and objectively. They acknowledge certain limitations of their work, nevertheless, and they may wish to note one or two others. First, the fact that Gua's relative intelligence as an ape was unknown makes it difficult to say whether the comparison of ape and child was, phylogenetically, fair. Also, Gua's age and that of Donald might have been more nearly equated. The X-ray showed that the ossification of Gua's bones at the beginning of the experiment made her structurally comparable to a child more than twice her age. She had all but her canine teeth, while Donald had only his two lower incisors.

There were other differences, physiological in nature. Gua was found to have a consistently higher metabolic rate and a lower pulse. The differences in blood pressure ranged from ten to twenty points. How much of this was individual in nature, and how much was representative of central tendencies for Gua's species had not been determined, and yet these are indices of organismic trends which, in the comparative study of behavior, should be carefully traced.

Aside from the fact that the ape, having spent 2½ months with her mother, had developed some fundamental habits which could not but exert a selective influence on her later habits, her superior physique from the start made her a somewhat less desirable subject for this experiment. Until some norms of ape-growth are obtained, and until these are correlated with those of child-growth (as determined by Gesell, for instance), behavior experiments such as this will not be as conclusive as they might otherwise be.

The sex of the ape is another variant of unknown value. It is of course now held that sex differences are not psychologically important. Physiologically, however, there are some rather crucial differences. It may be that Gua's sex did not enter very importantly into the determinations made by the Kelloggs. Since in an ideal experiment, however, all the factors that are standardizable are standardized or equated, it is safe to say that all future work will be considered inadequate unless it takes account, on general principles, of the subjects' sex, as well as of other factors, that can be properly controlled.

The Kelloggs have nevertheless produced a well-written, mature, and scholarly piece of psychological literature. Their book is full of careful and interesting observations, and fairly bristles with information of the type that might be of value to students of general and social psychology. Along with such books as Estabrooks and McDougle's *Mongrel Virginians* (or some other work written from the eugenic standpoint), on the one hand, and Itard's famous paper on *The Wild Boy of Aveyron*, translated by the

Humphreys, on the other, *The Ape and the Child* should be required reading in all courses where the problem of heredity and environment is discussed.

MAURICE H. KROUT.

FORTY YEARS OF PSYCHIATRY. By William A. White. New York and Washington: Nervous and Mental Disease Publishing Company, 1933. 154 pages. Price, $3.00.

Dr. White aims in his book to cover the history of psychiatry and show its development as he has seen it in the past forty years. Previous to this period the subject was unknown and entirely unexplored. It began to arouse interest and investigation just about the time Dr. White was beginning his practice as a physician. He has seen all the great developments in this field "unfold before his eyes" and has also participated in many of these developments. He has endeavored to give a complete picture of what he has seen and experienced both in the United States and in Europe, particularly at Munich. He gives the date 1893 as "a punctuation mark separating the age of ignorance and cruelty from the age of humanitarian endeavor" in the treatment of those who are mentally ill.

The field of psychiatry is one which cannot be ignored. Physicians, educators, clergymen, lawyers—men of every profession can benefit by a knowledge of the subject. Its rapid and almost phenomenal development alone indicates this as well as its wide application to problems of human nature that were previously untouched by scientific knowledge.

Dr. White's book is a worthy introductory source for any one. It covers the history and development of the movement thoroughly but without going into technical details and using terms that cannot be understood by the layman. For the most part the book is easy to read and sufficiently interesting to hold the attention. However, there are a few long quotations in fine print that are somewhat laborious to read and a number of paragraphs about the author himself which appear to be extraneous. The descriptions of the changes in method of dealing with the "insane" in hospitals should be very revealing to the uninitiated, especially the abolishing of restraint and sedative and narcotic drugs in favor of hydrotherapy, occupational therapy, etc.

Dr. White has given the layman what he wants to know—a survey of the field sufficiently detailed to picture the progress and present status of psychiatry. The method of presentation of his material would stimulate further investigation on the part of most readers and particularly those interested in human nature.

FLORENCE M. GEE.

NOTES

THE NEW YORK PSYCHOANALYTIC INSTITUTE

324 West Eighty-Sixth Street
New York City

The New York Psychoanalytic Institute offers the following extension courses during the academic year 1934–1935, beginning the first week in October, 1934.

(1) The Application of Psychoanalysis to Social Work—15 sessions.

An advanced course for executives in social work, experienced field workers, and visiting teachers of commensurate status. Approved for alertness credit by the New York State Education Department.

Leader: Dr. I. T. Broadwin.

(2) The Utilization of Psychoanalytic Viewpoints for Social Case Work—10 sessions. (Begins in January, 1935.)

An intermediate case discussion seminar for social workers.

Leader: Dr. Adolph Stern.

(3) Psychoanalysis in Medicine—15 lectures.

An introductory clinical course for physicians. (Not a training course.)

Lecturers: Drs. Daniels, Lehrman, Lorand, Meyer, Oberndorf.

(4) Parent-Child and Sibling Relationships (with special reference to maternal over-protection, maternal rejection and sibling rivalry)—6 lectures. General course.

Lecturer: Dr. David M. Levy.

(5) Popular Lectures on Psychoanalytic Topics—5 lectures.

Open to the general public. (To be announced.)

For information in regard to further details, apply to the Executive Director, the New York Psychoanalytic Institute, 324 West 86th Street, New York City.

CORRESPONDENCE

An announcement of a contest was recently mailed to departments of psychology by a magazine which purports to bring modern psychology to the general reader. The letterhead contained my name, among others, as a contributor to their pages.

I should appreciate the Journal of Abnormal and Social Psychology publishing this note to inform its readers that this use of my name was entirely unauthorized as well as grossly incorrect. I have never written for this magazine, but in spite of my refusal to contribute upon their invitation, find my name listed among their contributors.

It seems advisable, also, to call attention to the increase in "Cafeteria Magazines" which draw freely from the fields of psychology. This cognomen is appropriate since they help themselves to old writings of psychologists and publish these for readers of newsstand literature without either securing permission of the author or approval of alterations. Portions of a book, or of some forgotten article of almost any psychologist may be heralded across the country on their covers any month, much to the embarrassment of the original writer whose name will likely be in large type.

The best protection against this vicious borrowing is for the work to be copyrighted originally in the author's name. Most publishers will do this upon request, and producing psychologists should be urged to hold copyrights of their writings in their own name.

Definite developments in appropriating and garbling the works of psychologists has grown so in recent months that I believe this phase should be called to the attention of every psychologist who touches pen to paper and who is consequently exposed to the danger of becoming one of these injured yet innocent bystanders. The fact that several new publishing ventures of this sort are contemplated makes the situation somewhat acute.

Control can also be kept over the unwarranted use of one's work in advertising if the copyright is in the hands of the author. It costs only two dollars to copyright a book or article, and full information, together with simple application forms, can be obtained from the Register of Copyrights, Library of Congress, Washington, D. C. In the case of writings already published, transfer of copyright to the author can be simply effected; some publishers transfer automatically, while almost all will upon request.

In the general field of writing the problem of cafeteria condensations has become so acute that the Author's League of America has made special efforts to stamp out the evil. As this evil seems now to be headed strongly psychologyward, I am sending this note to two other journals in the expectation that it may save later embarrassment for many writing psychologists.

DONALD A. LAIRD,
Colgate University.

NEW BOOKS RECEIVED

(*This notice does not preclude the book from further notice or review.*)

Speech Disorders. By Sara M. Stinchfield. Harcourt, Brace and Company, New York, 1933. Pp. xii+341. Price $4.00.

Major Aspects of Personality. By Maurice H. Krout. The College Press, Chicago, 1933. Pp. xviii+364.

The Case for Sterilization. By Leon F. Whitney. Frederick A. Stokes Company, New York, 1934. Pp. v+309. Price $2.50.

Extra-Sensory Perception. By J. B. Rhine. Boston Society for Psychic Research, Boston, 1934. Pp. xiv+169.

The Ethics of Sexual Acts. By René Guyon. Translated by J. C. and Ingeborg Flügel. Alfred A. Knopf, New York, 1934. Pp. xxii+383 plus xxvii. Price $4.00.

Outline of Clinical Psychoanalysis. By Otto Fenichel. Translated by Bertram D. Lewin and Gregory Zilboorg. The Psychoanalytic Quarterly Press and W. W. Norton & Company, Inc., New York, 1934. Pp. vii+492. Price $5.00.

The Organism of the Mind. By Gustav Richard Heyer. Translated by Eden and Cedar Paul. Harcourt, Brace and Company, New York, 1934. Pp. xiii+271. Price $3.50.

Genealogy of Sex. By Curt Thesing. Translated by Eden and Cedar Paul. Emerson Books, Inc., New York, 1934. Pp. xii+286. Price $5.00.

A Doctor Studies Crime. By Perry M. Lichtenstein. D. Van Nostrand Company, Inc., New York, 1934. Pp. viii+263. Price $3.00.

Alcohol and Man. Editor, Haven Emerson and others. The Macmillan Company, New York, 1932. Pp. xi+451. Price $3.50.

Developmental Psychology. By Florence L. Goodenough. D. Appleton-Century Company, New York, 1934. Pp. xvii+619. Price $3.00.

How the Mind Works. By Cyril Burt, Ernest Jones, Emanuel Miller and William Moodie. D. Appleton-Century Company, New York, 1934. Pp. 336. Price $2.50.

La Psychologie Bergsonienne. By Roger-E Lacombe. Librairie Felix Alcan, Paris, 1933. Pp. 324.

The Science of Work. By Morris S. Viteles. W. W. Norton & Company, New York, 1934. Pp. xi+442. Price $4.00.

Introduction to Comparative Psychology. By Carl J. Warden, Thomas N. Jenkins and Lucien H. Warner. The Ronald Press Company, New York, 1934. Pp. x+581. Price $4.50.

Introduction to Physiological Psychology. By Graydon LaVerne Freeman. The Ronald Press Company, New York, 1934. Pp. xvii+579. Price $4.50.

Mental Defect. By Lionel S. Penrose. Farrar & Rinehart, New York, 1934. Pp. xi+205. Price $2.00.

To Be or Not To Be. By Louis I. Dublin and Bessie Bunzel. Harrison Smith and Robert Haas, New York, 1933. Pp. x+443. Price $3.50.

Mental Hygiene in the Community. By Clara Bassett. The Macmillan Company, New York, 1934. Pp. 394. Price $3.50.

Keeping a Sound Mind. By John J. B. Morgan. The Macmillan Company, New York, 1934. Pp. ix+440. Price $2.00.

Neuropathology. By Walter Freeman. W. B. Saunders Company, Philadelphia, 1933. Pp. 349. Price $4.00.